

Antiquity

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Editorial Notes

THESE Notes have more than once been written about primitive dwellings, but this is the first time they have been written *in* one. There is no better way of getting to understand any kind of house than to live in it for a time. This one is made of mud, size 15 feet 6 inches by 13 feet 9 inches, height about 12 feet. The roof is of twenty split palm-trunks, ten resting on each side of a single stout one supporting their ends. These beams support a closely set matting of wattles, which in turn support the outermost layer of mud. The floor is of sand or hard ground, and the walls are white-washed, about a foot thick and have four windows with wooden shutters. The room is furnished with wooden beds strung with local cord and two imported wooden armchairs. It opens sideways into a covered porch leading to an open courtyard. There are other rooms joined to this one by a wall and forming a square or rectangle.



This room is a unit in the courtyard type of house of which there must be thousands in the Nile Valley. It has no special peculiarities ; but it is interesting just because the type *is* so common. Simple as it is, far more thought has been given to it than the casual observer might suppose. Even the making of a mud wall 12 feet high that will stand up and not crack is not a thing that can be done without experience handed down traditionally. The whole thing is an excellent example of adaptation to local conditions—here consisting chiefly of heat and cold, wind and sand. The walls retain the heat during the cold nights—and they can be very cold in mid-winter ; and in summer you can sleep outside in the court.



The situation is at El Kab in the Sudan, at the top of the great bend where the Nile flows south-westwards. Rain rarely falls here, and when it does it is a disaster, causing some of the mud walls to disintegrate. This occurred in 1950 at Abu Hamed, 40 miles further up ; it would be interesting to know why some houses survived and whether it was due to any special structural features.

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However, I did not come to El Kab to study mud architecture, but to make plans of a couple of forts which, though mentioned by Cailliaud and Jackson, were practically unknown. One of them proves to be quite a sophisticated affair, mainly of rough stone ; it is roughly rectangular, with solid roundish bastions at the corners and in the middle of each of the longer sides. The walls are never less than 9 feet thick and still stand to a height of 12 feet or more ; the original height was much more because it was carried on in mud brick, traces of which survive in places. Inside the fort were round stone huts, some of whose interior walls were finished with a coat of plaster, painted red. Of the date it can only be said at present that it must be before A.D. 1500 ; later when the pottery (of which there is an immense quantity lying about) and the plans of the gateways have been studied, it may be possible to be more precise. Linant de Bellefonds made a sketch of the castles in 1822, and it proved very useful, as it shows features that have since fallen into ruin ; it is reproduced in my *Fung Kingdom*, Plate 18.



The excavation of such a site would be extremely difficult for all kinds of practical reasons, nor probably would the results justify the labour and expense. In this respect the stone fort of El Kab is typical of many sites in all parts of the world whose prime need is that they should be planned, photographed and described, and specimens of the pottery collected and placed in a museum for comparative study and for reference by future investigators. Making a plan is not technically difficult ; at El Kab it took four days, but that was chiefly because the mere task of walking about and taking measurements over an irregular mass of fallen stone was lengthy and laborious (especially with only a single untrained Bishari as assistant). But there are many compensations, apart from the satisfaction of doing the job at all. There is no better way of getting to know a site than to plan it ; one has to walk over and examine every portion, and one accumulates a really representative collection of sherds. One gets some idea of what the living conditions must have been like. And (here at any rate) one is rewarded by views of the majestic river, whose aspect changes from morning to evening but is always a delight to behold, and a never-ending wonder. Here it must be nearly a quarter of a mile wide ; a little higher up it is a mile wide, and in one place nearly two miles, set with islands and bordered by green crops planted on the steep mud banks as the water gradually falls.



There are many other sites to be similarly investigated between here and Atbara ; only a few are marked on the map, and not one of them in the whole stretch of 200 miles has ever been examined by an archaeologist. It is an exciting prospect, and by the time that these Notes are being read I hope to have examined many of them. The map marks the ruins of a church at one place (Kuddik), and Jackson has seen another, with granite columns, now incorporated in a mosque (Artul Island). There are rock-pictures and castles of unknown age and character ; and there is at least one early neolithic site (Gereif). There is also of course the probability of lighting unawares upon some important new site.



Exactly one month has passed since I wrote the last sentence, and the proofs have reached me in another rather remote place. The forecast has been abundantly fulfilled. Several more of the stone castles have been found and planned. Beside two of them

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are churches built of red brick, the walls still six and ten feet high. Many of the pots used by the castle-people had crosses incised just inside the rim. Christianity was the religion of the Sudan for at least 700 years ; it is usually supposed to have yielded place to Islam soon after A.D. 1300, but the next two centuries are historically blank and it may have lingered on longer in certain regions. In one place just outside a small Christian village were the flattened remains of tombs that were covered with a cement-like plaster and may have been precursors of the Mahometan *gubbas*. On some of these I found broken tiles with inscriptions in Greek letters. These, though fragmentary, should help to provide a much-needed date for the period of the castles. That is a job for experts.



It is pleasant to think that two months still remain for more discoveries. Enough has been done already to show that there is a rich yield for field-archaeology in these parts, and that the principles thereof learnt in Britain are equally applicable here. They are in fact of world-wide validity. The art of interpreting superficial features demands, apart from practical experience, little more than keenness of observation, especially of minute and apparently trivial detail, combined with common-sense and imagination. Perhaps one might add patience and enthusiasm. It is desirable to work from a centre rather than to do a long trek ; a few sites done thoroughly—photographed, planned and described—are better than many observed casually in passing. While travelling one can never spare enough time to examine a site thoroughly ; conversely, while planning it one gets to know it thoroughly, and one discovers new features almost every day.



Some readers may be wondering whether work of this kind in the Sudan is affected by the political situation. It is not affected by it in any way at all, nor does it seem likely that it ever will be. Everywhere one meets with the greatest friendliness and hospitality, and if the transition to self-government is effected in an orderly manner, as one hopes, there seems to be no reason why future investigators of antiquities should not do their work as comfortably as the present writer is doing his.



It is only proper that these Notes should conclude with a tribute of sincere thanks to the British Academy whose generous grant has made it possible for them to be written. Some account of the expedition is to be given to them in a lecture next November.

Sutton Hoo? a Summary

by THE EDITOR

MORE than a decade has passed since the ship-burial at Sutton Hoo was excavated, but owing to the war serious study of the finds has been of much shorter duration. Even so, in spite of much intensive research in laboratory and study, all of first-rate quality, certain vital problems still remain unsolved. We still do not know in whose honour the cenotaph was made; though we can say 'with complete certainty that no body, either cremated or inhumed, ever occupied the "body-space" at the west end of the burial-chamber, where everyone is agreed the primary burial should have been¹'. Before the king can be identified the date must be determined, and that in turn may depend to some extent upon the coins. Since I first discussed this matter², further pronouncements have been made by coin experts³, but there has still been no full and frank statement of the evidence upon which the date (A.D. 650-70) has been arrived at, and it is now long overdue. As a layman who has already somewhat rashly broken a lance with the coin experts, I feel that unless we are soon given something more substantial to discuss, the case for the date 650-70 may be said to have been lost by default. Dr Gordon Ward seems to think that that has already happened. The critical article of his which is printed below assumes a knowledge of Mr Bruce-Mitford's in the *Proceedings of the Suffolk Institute of Archaeology*⁴; for this reason, and because of its intrinsic importance, I have summarised it and printed it here. The summary has been submitted to Mr Bruce-Mitford and is approved by him as representing his own views correctly. Some of his arguments assume that the date of 650-70, handed out by the coin experts, must be provisionally accepted. But most of them would be just as cogent, and his main conclusion equally valid, if the date should eventually be put back about a quarter of a century and the cenotaph assigned to Redwald. O.G.S.C.

SUMMARY OF MR BRUCE-MITFORD'S ARTICLE

The purpose of his article was to clarify the broad position of the Sutton Hoo burial in English history and archaeology.

Some of the objects found resemble very intimately objects found in Sweden, thus indicating (for the first time) that there was a direct link with that country. This is agreed by all, but its meaning depends upon whether the burial was made for a stray Swede or for an established English king; and before even that can be discussed we must decide whether it was made for a king or not, whether it is Pagan or Christian, or whether it was a grave or a cenotaph.

¹Bruce-Mitford, 42, quoting references.

²ANTIQUITY XIV, 1940, 64-8. I see no reason to withdraw anything there.

³See *B.M. Provisional Guide*, 1947, 42: otherwise unpublished.

⁴Vol. 25, part 1, 1959; sold separately (78 pages; price 5s 3d including postage) by the Society at the Ancient House, Buttermarket, Ipswich.

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IS THIS A KING'S BURIAL?

Yes. As Professor Chadwick pointed out kings were the repositories of all treasure and no one but a king could have possessed such wealth as was found. A *cenotaph* on this scale must surely have been for a king. The spoons and the ten (imported) silver bowls were probably royal gifts and, together with the other silver objects and those others (whose existence we may assume) which were not buried at all, are unlikely to have been accumulated except in a royal treasury and family. The theory that it is the cenotaph of a distinguished foreign visitor, though plausible, is unlikely to be correct, and seems disproved by the presence of certain objects (such as the whetstone and standard) which are claimed to be the prerogatives of the king. The whetstone is unused and certainly ceremonial, for it is unusable. 'It is a unique, savage thing, and inexplicable, except perhaps as a symbol, proper to the king himself, of the divinity and mystery which surrounded the smith and his tools in the northern world' (Kendrick). The iron standard was at first explained otherwise, but none of these other explanations can be sustained. Standards were the symbols of royal office peculiar to the Anglo-Saxon background, and ours has therefore an important bearing on the nationality, and so on the identity, of the person here commemorated. A standard was used by the Saxon Bretwalda (Overlord or High King) Edwin, who was also King of Deira (616-32) and had close relations with Redwald and the East Anglian court. Like the title Bretwalda the standard may well have a Roman lineage; and we know that the East Anglian dynasty of Wuffingas claimed a Roman Caesar ('Caser') as one of their ancestors. Standards are shown on some of the earliest Anglo-Saxon coins, including a gold one of the 6th century which imitates one of the emperor Honorius. 'If the use of such a standard is thought of as part of a Bretwalda's ceremonial, then our object was no doubt made for Redwald, and passed on to his successors in the reduced days of East Anglian power, as a legacy from his reign'⁵

IS THE BURIAL PAGAN OR CHRISTIAN?

Though grave-goods were buried in Christian graves and the custom was tolerated by Christian authorities, it was regarded as inefficacious rather than wrong and therefore tended to die out, the grave-goods assuming more and more a token character. The extravagance of Sutton Hoo is unlikely to have occurred in Christian times. The arguments of Professor Lindqvist, protagonist of the 'Christian' case, are shown to ignore the historical setting and to be untenable. But by 650, 'the earliest date considered on numismatic opinion to be possible for the Sutton Hoo ship-burial, East Anglia was substantially converted'; so that the great burial must represent either a reversion to paganism, or the accession of a king who, even at this late date, still remained faithful to the old gods. Both alternatives are possible but open to objection. Any general reversion would surely have been mentioned by Bede, who records such elsewhere. If a pagan king has to be found, Aethelhere (d. 655), ally of the pagan Penda who had killed Aethelhere's most Christian predecessor Anna, is the most likely; his death on a distant battlefield seems to fit admirably with the circumstance of a cenotaph.

But if it is the burial of an obstinate Pagan, how can we explain the 'positive and striking evidences of Christianity' revealed by the objects found in the burial, such as the spoons with their patently Christian inscription and the crosses on the scabbard-bosses? How can we explain the presence of such significant Christian objects, concentrated, intentionally as it seems, in the body-space of the burial-chamber, if the person

⁵ But if we accept the premise, is not the case for the cenotaph being Redwald's greatly strengthened? O.G.S.C.

concerned was in fact a pagan? If we accept the date 650-70 we have to find some East Anglian king who satisfies these conditions. The only ones, it seems, are Aethelhere and Anna. There are some indications that Aethelhere may have been a pagan. But it is preferable to regard the Sutton Hoo ship-burial as 'a public and traditional monument erected in honour of a notable king [Anna] whose body had received Christian burial elsewhere'. A similar cenotaph occurs at Jellinge in Denmark, erected for a royal convert whose body was actually buried in a church; but in this case there were no grave-goods deposited. Anna died in 654, and is known to have been buried at Blythburgh. He was an outstanding king and the most likely one to be found within the given date-limits.

THE SWEDISH CONNECTION

Any interpretation of this aspect must be provisional, pending the definitive description of the major objects and the reconstruction and study of the less spectacular ones, which has not been carried out. These more modest objects may yet be archaeologically the most revealing. It must also await the excavation of the seven still intact barrows of the same group, and the publication of the finds from the three others.

Similarities between the Sutton Hoo material and Swedish finds of the Vendel period were noted as soon as the Sutton Hoo pieces were excavated, and appeared more striking when the shield and helmet were restored in 1946-7. But before drawing any conclusions from these similarities we must be sure that they are not merely the elements common to a widespread culture. For close parallels may also be found in the Rhineland and in Italy; similarities in gold cloisonné work have been pointed out, and if such were the work of itinerant craftsmen, no significant connection with Sweden need be inferred from them. We find, however, that the similarities occur over a broad front and consistently, not merely in goldwork, so that we pass beyond the stage of isolated and dispersed parallels. (This is proved by a detailed analysis of several objects and of the art-motives depicted, which is hard to summarize and therefore omitted). It must also be noted that the two cultures also have a *custom* in common—that of boat-burial, which in the 7th century occurs nowhere else in Europe outside Suffolk and Uppland in Sweden. Thus the resemblances cannot be explained in terms of gifts or trade but betoken a more substantial link. Independent parallel development from a hypothetical common continental source may be ruled out. There was a direct and substantial connection between East Anglia and Sweden a hundred and fifty years and more before the first Viking raids. It seems to have been with Uppland, that is, with the kingdom of the Svear rather than with that of the Geats.

THE INTERPRETATION OF THE SWEDISH CONNECTION

The view that practically the whole of the Sutton Hoo treasure, including the jewellery, was imported from Sweden is rejected by all Swedish archaeologists and is untenable. For instance, chequered inlays of millefiori enamel do not occur in northern or continental metal-work, and are an insular phenomenon derived ultimately from a technique employed here in Roman times. It occurs in the escutcheons of the hanging bowls found in Anglo-Saxon graves. The use of millefiori at Sutton Hoo proves that the objects thus ornamented were made in England. Analysis also proves that all the other pieces (except the sword-pommel) are products, if not of the craftsmen who made the major pieces, at any rate of the same workshop or milieu.

The theory of a pagan Swedish king who invaded and conquered this region in or about 670 is ruled out by our knowledge of contemporary East Anglian history—which

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is much more extensive than Professor Nerman, author of the theory, seems to realise. Such a conquest runs counter to the whole tenor of Bede's narrative and could in any case hardly have been passed over without mention by Bede who was quite well informed about East Anglia at this period. (Professor Nerman's theory is discussed in detail and decisively rejected). We must therefore find some other explanation that fits the facts better. What we have to account for is 'the presence in an East-Anglian King's cenotaph of Swedish objects that were ancient when buried'. We have to explain the active Swedish influences at work as revealed in the regal East Anglian grave-goods; and the following of Swedish usage for the funeral rites of an East Anglian king—if it be allowed, as it must be, that boat-burial may be a Swedish element.

'It seems to me that there could be one simple explanation, which would explain everything, and is positively suggested by the archaeological evidence as we have it at present. All the phenomena in the Sutton Hoo ship-burial that we have discussed are perfectly explicable if we suppose that Nerman's Swedish conqueror established his control over East Anglia not at the period of our burial—mid 7th century—but a good deal earlier; in fact, that he was the man who founded the East Anglian dynasty in the mid 6th century. What we find in the burial would then be Swedish heirlooms (sword, helmet, shield) treasured as symbols of the origin or history of the royal house, and, in things that were made in East Anglia for Wuffinga patrons, traces of Swedish ideas and influences that would naturally spring from such roots. But by A.D. 650-70, the time of the Sutton Hoo burial, the dynasty having been established for a hundred years, or some four generations, would have become wholly absorbed in its Western European milieu and an integral part of Anglo-Saxon civilisation. Hence the perfect manner in which the burial as a whole fits into its insular setting. Once such a family link had been established between East Anglia and Sweden, we may suppose that contacts were from time to time renewed'.

Statements in the sagas quoted by Nerman that a Swedish king conquered a fifth part of England (described as Northumbria) would thus be seen to have some basis in historical fact, though attached to the wrong king and the wrong part of England. Since nothing is known of the origin of the Wuffinga dynasty there is no reason why we may not suppose that they came from Sweden about the middle of the 6th century. There are good reasons for thinking that, if so, they were an offshoot of the royal house of Uppsala, the Scyldings.

This explanation is substantially the same as that advanced by Professor Lindqvist, but was reached independently, and on broader grounds and a more detailed consideration of the evidence. Be it noted that one of the other barrows in the group at Sutton Hoo contained a boat. Some of the others may prove to have contained boats too. 'In the Swedish boat-grave fields, the boat-burials are only accorded to the heads of the family and they are accordingly strung out in time, about a generation apart. If the analogy with the Swedish boat-grave fields is to be pressed, the likelihood is that, if other boat-graves do exist, some of them will be appreciably older than the great ship-barrow. If boat-burial is a Swedish custom, its introduction into England would thus have taken place considerably before 650-70. This would imply that the contact with Sweden goes back into an earlier period'.

The repairs made to certain gold cloisonné buckles and mounts in the ship-burial show that they were of some age when buried. If these were English objects used to form a harness for a Swedish sword, we must recognise a Swedish cause operating an appreciable time before the Sutton Hoo burial.

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The shield was old and dilapidated when buried, and is Swedish ; it would hardly have been brought over from Sweden in that condition and therefore probably fell into disrepair here. This again must represent Swedish contacts much earlier than the date of its burial.

The helmet, too, is Swedish and has been dated ' not much later than the Anastasius dish ' (491-518). This dating, though well grounded, must be regarded as ' no more than a strong body of opinion ' ; but the helmet certainly ' stands closer to late Roman prototypes than any of the Vendel or Valsgärde helmets, and . . . has also suffered some damage and been repaired ' ; so that it should be at least as old as the date suggested for the shield (*c.* 600 or earlier).

' In these ways the archaeological evidence conveys the impression not only of a pervasive Swedish influence at the highest social level, but of such influence extending back into the earlier days of East Anglia and its royal house. From what we know of the dynasty, much the most probable time for the introduction of such an influence would be at its inception '.

The Silver Spoons from Sutton Hoo

by GORDON WARD

THERE were found in the great ship burial two silver spoons, ten inches long, of a type well-known about the 6th century A.D. They have inscribed on them in Greek the names SAUL and PAUL (FIG. 1). Although several spoons of this character are known to students, their precise use remains obscure. Since they mostly bear the names of saints it is possible that they served some special liturgical purpose, but it is equally possible that they were strictly comparable to the christening mug of later ages (see discussion by Ernst Kitzinger in *ANTIQUITY* for March 1940). At Sutton Hoo the spoons were closely associated with a set of silver bowls bearing a cruciform decoration and were placed close to the right side of the position which should have been occupied by the head of the deceased. R. L. S. Bruce Mitford of the British Museum (*Proceedings of the Suffolk Institute of Archaeology*, Vol. xxv, 1949), writes as follows:—‘We may accept the spoons without hesitation as a present for a convert—not a Christening gift for an infant, but as a gift intended to mark the baptism of an adult convert relinquishing his pagan state, and no doubt a royal convert’.

The long and important paper quoted above (and summarised by the Editor in the preceding article) is described by its author as embodying the official views about Sutton Hoo and thus claims very particular authority. It is in fact full of most valuable facts and helpful suggestions and will often be made use of in what follows. But its final decision that this is most probably the grave of King Anna or King Aethelhere seems to be without sufficient support. On the contrary, I believe that a careful consideration of these silver spoons and the associated silver bowls would lead us back to the view that this was Redwald's grave.

Bruce Mitford's reasons for regarding these spoons as baptismal gifts to an adult are that they obviously refer to a particular occasion in the life of St. Paul, namely, his vision on the road to Damascus, after which the orthodox Jew Saul became the Christian Paul. Since such spoons were usually inscribed with the names of the saints the inclusion of ‘Saul’ (who was certainly not a Christian Saint) can scarcely be explained on any other hypothesis. We have therefore to look for a King of East Anglia who might have received so unusual a baptismal gift, and so valuable a present, as the silver spoons, together with the adjacent set of silver bowls. Of the kings who might be in question:—

1. Redwald was baptised in Kent early in the 7th century.
2. Earpwald was converted by Edwin of Northumbria, and presumably baptised there about 627–8.
3. Sigberct was baptised in France sometime before he came to the throne, about 630–1.
4. Anna and his successors were no doubt baptised in East Anglia for Bishop Felix was there from about 635 to 654 and thereafter there was always a Christian centre in East Anglia.

One further point needs to be considered before we discuss which of these kings might first have owned the silver spoons. Bruce Mitford (*loc. cit.*) tells us that silver

of this sort is excessively rare in Germanic graves, writing as follows :—‘ The fact remains that classical silverware, whether acquired by ecclesiastical gift or continental trade, is excessively rare not only in Anglo-Saxon but in Continental Germanic archaeology of this period. I can quote only the *phalerae* [a term covering various ornaments for men, women and horses. G.W.] from Ittenheim as instances of Mediterranean silver in the classical tradition occurring in a Germanic grave of this period (6th–7th centuries)’. This shows that Merovingian influence, which was reasonably strong in England during the 7th century, did not extend to the bringing in of this sort of silver which would not, in any case, commend itself to Merovingian ideas of true art. Indeed, when they looted silver in this form, the various barbarians who eventuated later in the royal races of France, etc., probably melted it down and converted it into torques or bracelets. We may deduce from this that the silver at Sutton Hoo was derived from the Mediterranean countries and perhaps from Rome itself. Bearing this in mind we may now consider further the list of baptisms given above. It is at once obvious that only Redwald is likely to have received a rich baptismal gift derived from the Mediterranean area. We know that he was baptised in Kent, possibly by St. Augustine

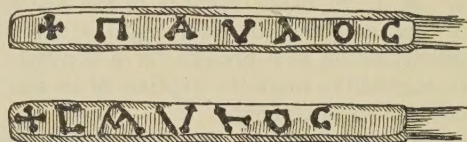


FIG. 1. INSCRIPTIONS ON THE SILVER SPOONS
FROM SUTTON HOO

(Block kindly lent by the Suffolk Institute of Archaeology)

himself, and we also know that the Pope sent rich gifts to the Kentish King. It may well be that some of these gifts were handed on to Redwald. Although he was then a young man his conversion was a matter of the greatest importance. The Augustinian mission had met with very little success outside the immediate sphere of Aethelbert's influence. It had failed with the Celts, and Sussex remained pagan for long afterwards. The Pope's intention that an Archbishop should be set up in London and another in York was utterly impossible of realisation. The conversion of East Anglia would at least have been a step towards a further advance and as such was certainly to be regarded as very greatly to be desired.

There was a further and even better reason for trying to enlist the goodwill of Redwald. His kingdom of East Anglia was increasing in power and began to threaten the predominance of Kent. Bede assures us that even in Aethelbert's lifetime Redwald began to contend with him for the Bretwaldaship. This may of course have been some time after his baptism but Aethelbert would be well aware of the threat long before it eventuated in specific action. In these circumstances the baptism of Redwald was a considerable political as well as religious triumph and is absolutely certain to have been marked by the presentation of exceptionally valuable Christening presents. We know that such gifts were available. It may therefore be deduced with considerable confidence that the silver spoons of Sutton Hoo (and perhaps also the silver bowls) were baptismal gifts to Redwald which he had carried back with him to East Anglia. This is advanced as a rational and not unsupported deduction explaining why this particular Germanic

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grave was found to contain silver articles of a sort almost unknown in others. The evidence of the silver spoons therefore points directly to Redwald, and there is still further evidence which points in the same direction, that is :—

1. The very unusual richness of the grave goods is agreed by all to suggest the burial of a great king and it is known that Redwald was far and away the greatest of East Anglian kings. He was the only Bretwalda in the family. He had not been afraid to befriend the exile Edwin when no other kingdom dared tolerate his presence, and he had later defeated the powerful king of Northumbria at the River Idle. He had before his death successfully contended with Kent. No other East Anglian king reached anything approaching the eminence of Redwald. It is true that we lack details of his predecessors Tyttla and Wuffa but the coin evidence excludes both of them, if not from great eminence at least from burial in this particular barrow at Sutton Hoo.
2. The large size of the mound, the largest in the group, and of the contained ship, are further evidence that here was buried a man of very great importance.
3. The fact that the burial was not in or near a church seems to rule out all but the pagan kings, Redwald, Ricbert the usurper and possibly Aethelhere, of whose religion we have no information. Redwald is the obvious choice from these three.
4. The presence of Christian objects in a pagan grave fits admirably the case of Redwald, who had both Christian and pagan altars in his place of worship to the end of his life. It fits no one else.
5. The other features of the burial are also such as we should expect if Redwald was buried here, e.g., the great Swedish shield so old that it had to be repaired before it could be used in the funeral ceremonies. They can only be harmonised with any other king by very specious reasoning.

Finally it may be noted that the iron standard provides a very suggestive link with Redwald. This requires more detailed discussion. We have practically no knowledge of the standards used by Anglo-Saxon monarchs except that Bede provides a very welcome sidelight on the grandeur of Edwin of Northumbria. He notes that Edwin had three different sorts of standard. In battle a banner was borne before him and when he went through the streets there was carried in front of him what the Romans called the 'tufa'. We are not concerned with these. The third type of standard seems to have been used on more ceremonial occasions, that is, when Edwin was riding with his Court 'between cities or villas or provinces'—which means when he was paying official visits. On such occasions, Bede says, 'semper antecedere signifer consuesset'. This means that a standard bearer used always to go before him, which may either imply that he rode in front of him or that he preceded him to the appointed place and there set up the standard. The sort of standard which was borne by a 'signifer' in Roman times is well-known. It had a metal point at one end for sticking into the ground and its upper part was elaborated with metal work and devices subsidiary to the main emblem at the top. In fact, such an iron standard as was found at Sutton Hoo falls readily into the group of standards borne by a signifer.

It is easily assumed that Edwin of Northumbria invented his own three standards after examining classical examples on coins, in manuscripts or conveyed to him by word of mouth. It may even be true that some of these sources were open to him, although this is rather difficult to believe since he commenced his reign as a pagan and, as far as we know, quite out of touch with Mediterranean civilisation. It is much more likely

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that he had seen in use, in East Anglia, this same iron standard from Sutton Hoo surmounted by its representation of a stag, for Edwin and Redwald were old friends. Edwin as an exile had spent a long time at Redwald's Court and must have been well aware of its usages. It was natural enough that he should introduce them in Northumbria when his years of good fortune came along. There may yet be found from Sutton Hoo the remains of a banner or tufa, to bear the standard company.

We can hardly be asked to suppose that the two kingdoms of East Anglia and Northumbria arrived quite independently at the idea that they ought to have standards carried before their kings. It is much more reasonable to believe that the idea spread through the known link between the two, the Edwin-Redwald link prior to 616. Are we then to believe that this is after all the grave or cenotaph of King Redwald? The answer would be an unhesitating 'Yes' if it were not for the alleged evidence of the coins. This has been allowed an importance which the facts in no way justify, and it is with regret that one reads Bruce-Mitford's statement that the year 650 is 'the earliest date considered on numismatic opinion to be possible for the Sutton Hoo ship burial'. As a matter of fact the numismatists have altogether insufficient dated material to enable them to give any confident opinion about Sutton Hoo, and they do not hesitate to say so.

There were forty coins in the purse. One of these seems to be that of a king Theodebert who reigned from 534 to 548. It is a forgery and might possibly be of later date. It should be remarked that forgery was very common about this time, probably because the official mints failed to meet popular needs. Three other coins, which were also imitations, bore the name of the Emperor Maurice Tiberius (582 to 602). These first four coins suggest a date for the burial quite early in the 7th century. The remaining 36 coins had no identification marks except the names of the towns at which they were struck and of the moniers who struck them. As it is reckoned that there were at least one thousand Merovingian mints in operation at this time these details are of no help to us. There were two coins in the hoard identical with some of the coins in another hoard which is thought to have been buried at Bordeaux not long after 670, and it is also said to be the fact that most of the 36 coins are of a sort which were current during the reign of Clovis II (A.D. 638 to 657). These two observations only serve to show that gold coins of this sort might remain in circulation for a number of years. They do not really assist when it comes to the question of dating the ship burial.

We will now consider what the experts have said about these coins. They were first of all examined by Mr Derek Allen of the British Museum who concluded that the Sutton Hoo burial 'cannot have taken place before about A.D. 600, is not likely to have taken place earlier than A.D. 640 to 650, and might even have taken place nearer A.D. 670'. He would not, however, allow that the coin evidence definitely excluded the possibility that this was the grave of Redwald although he thought that it suggested a somewhat later date. Monsieur le Gentilhomme admits that the burial may be dated 'at the beginning of the second third of the 7th century' which would be soon after 633. Redwald probably died somewhere about 625. Finally we have the opinion of Mr Dayrell Reed who states that 'the only evidence supplied by the bulk of the coins is that they might have been struck at any time between 615 and 675'. He concludes that 'the jewellery, the silver and the historic facts all support a date of about 625 and only by straining the numismatic evidence can that be made to oppose it'. Mr O. G. S. Crawford, reviewing these opinions in *ANTIQUITY* for March 1940 reaches the conclusion that 'where such a range of possibilities exists, a difference of five or ten years cannot be detected; and that is all that separates the numismatists from their archaeological and historical colleagues'.

THE SILVER SPOONS FROM SUTTON HOO

It is evident that the numismatists have been set a task for which they are not equipped. They have no really accurately dated coin of the period under review. King Clovis reigned for 20 years and doubtless possessed coins which had been in the royal treasury long before that. A comparison between his treasure and that at Sutton Hoo helps not at all ; yet this is the only sort of evidence which is available for anything like close dating. Moreover, the numismatists have very, very few examples which can be used for purposes of comparison. It is no fault of theirs if their conclusions are hesitant and not generally agreed or acceptable. In any case, it can be said with confidence that none of those quoted would dare to assert that it was impossible that the purse at Sutton Hoo can have belonged to king Redwald.

The final conclusion is therefore that same conclusion to which Professor H. Munroe Chadwick came in 1940, which he expressed as follows :—‘ If the burial dates from 600 to 640, all probability is in favour of the great and wealthy high King Redwald, who seems to have died about 624-5. He may possibly have buried the objects himself at the funeral of a relative ; but what we know of the personnel of the period suggests that his wife was the person responsible, and consequently that the person commemorated was probably Redwald himself ’.

No single numismatist has come forward to say that the burial cannot date between 600-640 and it is still for those who say that this cannot be the grave of Redwald to prove their thesis.

The Continental Home of the English

by H. JANKUHN

(Kiel)

[In April, 1938, I had a short holiday in Schleswig, chiefly in order to see and walk along the Danewerk. During my stay there, Professor Jankuhn took me to see the province of Angeln; and on a little hill above the small town of Süderbrarup we inspected the much disturbed remains of a circular stone-setting, perhaps once covered by an earthen burial-mound (barrow). The spot is marked B on the plan (FIG. 7), and is to be seen also on PLATE I. It was for centuries the meeting-place of the district, at which discussions were held, just as at our early Hundred Courts, to which it may have corresponded. Here, said Dr Jankuhn, the Angles of Angeln must surely have met to discuss their forthcoming expeditions to England. It is seldom possible to nail down decisive historical events to a particular spot; and this revelation seemed to me most dramatic and would, I said, make an admirable article for *ANTIQUITY*. But other events supervened, and this earlier invasion receded into the background. Now at last the article has come into being, not quite perhaps in the form originally planned, but none the less welcome. O.G.S.C].

ARCHAEOLOGISTS now regard it as certain that the Saxons who took part in the settlement of the British Isles in the 5th and 6th centuries A.D. came from the region between the Elbe and the Weser; but the continental home of the Angles is still, rather strangely, a subject of controversy.

Bede states unambiguously that the Angles came from the region called Angulus, which lies between the Schlei and Flensburg¹ and still bears the name of Angeln. Ptolemy put the Angles much further south on the middle Elbe, next to the Langobards.² Tacitus, in his description of the peoples of Central Europe mentions the Angles as members of a cultural community united in the worship of the goddess Nerthus; but he gives no clue to their area of settlement.³ On the basis of this evidence, and some minor written sources, three theories have been put forward about the origin of the Angles. The theory that the name Angli was developed autochthonously in England needs no discussion.

Besides the historical sources we also have, as aids in the solution of the problem, the Sagas, the development of the language, place-names and archaeological finds.

The examination of the Sagas starts with the Offa tradition⁴—that this Saga reflects facts of Anglian history on the continent, and that it came to Britain with the immigrating Angles⁵. The geographical context there is given by the proximity of the Danes. The

¹ Bede I, 15.

² Th. Steche, *Altgermanien im Erdkundebuch des Claudius Ptolemäus*. Leipzig 1937.

³ Tacitus, Germ. cap. 40.

⁴ G. Schütte, *Gotthiod und Utgard*. Kopenhagen 1935 and 1936. Herm. Schneider, *Germanische Heldensage*, two volumes, Berlin 1928, 1933 and 1934. Siegfried Gutenbrunner, *Schleswig-Holsteins älteste Literatur*. Kiel 1949.

⁵ Jnger N. Boberg, 'Die Sage von Wermund und Uffe'. *Acta phil. Scandinavica* 16, 1942, p. 129 sequ. and G. Schütte, *ibid*, p. 233 ff.

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Saga of Offa is, however, unknown amongst the German heroic Sagas, so that it could not have originated in, say, Thuringia on the middle Elbe. All the clues to the localisation of the Offa Saga point to Schleswig as the place of origin of the oldest Offa Saga, and so indicate Schleswig as the home of the Angles, whose king was Offa.

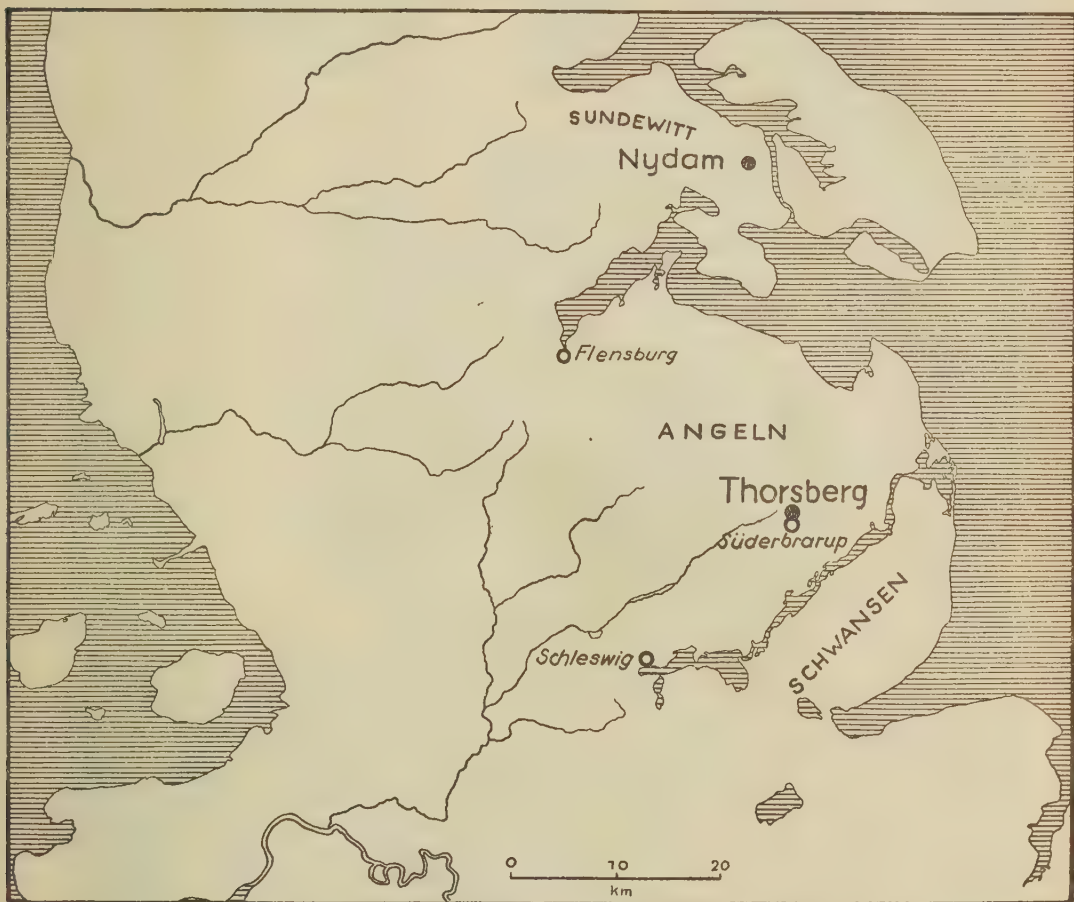


FIG. 1. CENTRAL SCHLESWIG SHOWING POSITION OF NYDAM AND THORSBERG

Philologists consider that Old and Middle English traditions and modern English dialects point to Schleswig or Jutland, lying to the north of the old Saxons, as the continental home of the Angles⁶. There is no evidence from the place-names of A.D. 500 and onwards of any special connection between Thuringia and the Anglian settlements

⁶ R. Jordan, *Eigentümlichkeiten des englischen Wortschatzes*. Heidelberg 1906. E. Schwarz, 'Das angelsächsische Landnahmeproblem. German-Roman'. *Monatsschrift*, 32, 1950, p. 44.

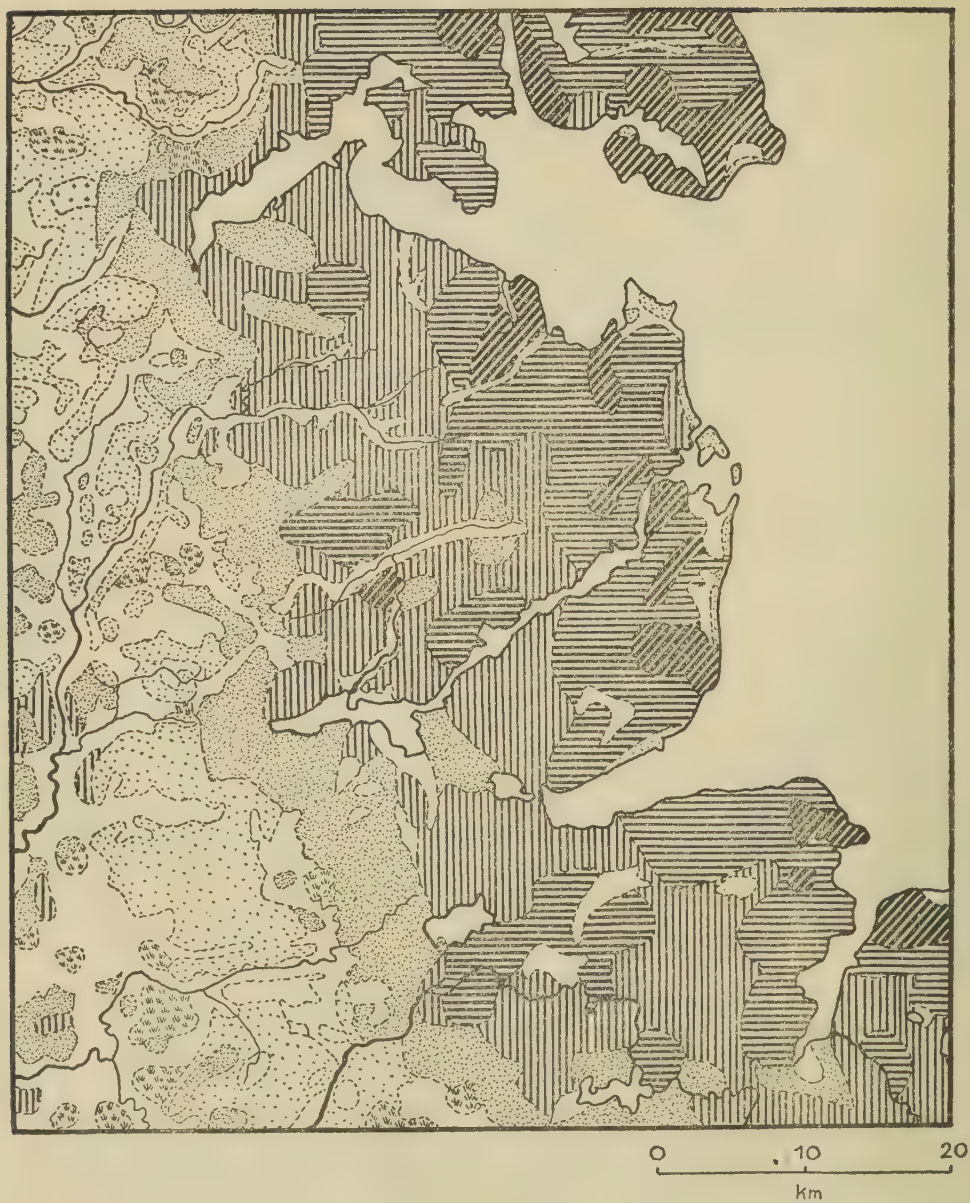


FIG. 2. SOIL MAP
Horizontal and oblique lines=heavy clay. Vertical lines=sandy clay. Dots=clayey sand

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FIG. 3. CEMETERIES OF THE PRE-ROMAN IRON AGE (2ND AND 1ST CENTS. B.C.)

in England. But a few isolated place-names in Schleswig, such as Lipping in Angeln, do recall similar names, such as Lipping, Leicestershire, in England⁷.

So far as the archaeological evidence goes, a recent examination of the finds from the middle Elbe region during the first six centuries of the Christian era has shown no evidence of any connection with England during the 5th and 6th centuries⁸. Moreover, enough is known of the history of this Elbe region, where Ptolemy put the Angles, to show that it was almost uninhabited during the 1st and 2nd centuries A.D.⁹ In any case there is no evidence that there was any large emigration from here across the North Sea during the 5th century.

The history of the settlements in the Angeln region has been studied intensively during the last 20 years¹⁰. It is found that, during the Bronze Age, the regions of Angeln and Schwansen were densely populated, the population appearing to become less dense towards the end of the period, as shown by the increasing scarcity of finds. There are no burials of the Early Iron Age recorded for either region, though plentiful in adjacent regions to the north and south. In the 2nd century B.C. cemeteries appear again, occurring not on the (mostly heavy) clay soil but restricted to sandy patches within the heavy clay area. (FIGS. 2 and 3). The finds from these urn-field cemeteries are identical, especially in regard to the pottery, with those from the region between Hamburg Lüneburg and Schwerin¹¹. Other typical implements and ornaments, such as the 'Holsteinische gürtel', point to southern Holstein as the place of origin of these new inhabitants of Angeln¹². Furthermore, there appear, though in smaller numbers, urns¹³ and ornaments of a type common in Jutland, and in Southern Sweden. If this were not merely due to trade, it must mean that some of the new colonists came from the north. There are also some special native forms which developed in Angeln during the 1st century B.C.

The archaeological evidence suggests that the numerous Bronze Age folk of Angeln emigrated, possibly driven out by a worsening climate, and that both Angeln and Schwansen remained uninhabited for several centuries. To test whether these regions really were abandoned at the end of the Bronze Age, a method was used whereby the history of corn-growing can be determined by means of grain-pollen preserved in peat-bogs. Since agriculture had already been established for many centuries in Central and Northern Europe, periods of dense population should be revealed by the pollen-graph by a great abundance of grain-pollen, and periods of depopulation by its absence. Palaeobotanic research carried out in two bogs confirmed this inference, showing that agriculture had almost ceased at the end of the Bronze Age¹⁴. The absence of burial-finds corresponds nicely with a cessation of agriculture.

⁷ Kristian Hald, *Stednavne i Angel. Knudsen-Kretzschmer, Sydslesvig* II. Angel, Kopenhagen 1945, p. 70 ff. W. Laur, 'Angelns älteste Ortsnamen'. *Jahrbuch des Angler Heimatvereins* 14, 1950, pp. 133-40.

⁸ H. Jankuhn, 'Zur Frage nach der Urheimat der Angeln', *Ztschr. d. Ges. für Schlesw.-Holst. Gesch.*, vol. 70-1, 1943, p. 20 ff.

⁹ Fr. Kuchenbuch, 'Altmärkische Funde des 1 und 2'. *Jahrhunderts n. Chr. Sächsisch-Thüringische Jahresschrift* 1936, p. 211 ff.

¹⁰ H. Jankuhn, 'Siedlungs- und Kulturgeschichte der Angeln vor ihrer Auswanderung nach England'. *Jahrbuch des Angler Heimatvereins* 14, 1950, pp. 54-132.

¹¹ *ibid*, plate I, a, b.

¹² *ibid*, plate I, c.

¹³ *ibid*, plate I, e.

¹⁴ By Dr Schüttrumpf, Kiel.

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FIG. 4. CEMETERIES OF THE ROMAN PERIOD

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But it seemed desirable to carry out some kind of test, to find out whether this cessation of agriculture might not have been due to some change in farming economy caused by a deterioration in the climate rather than to an abandonment of the Settlements.



FIG. 5. SITES OF 1ST AND 2ND CENTS. IN SCHLESWIG-HOLSTEIN (after Tischler)

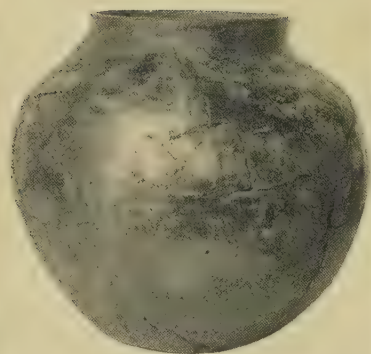
With this object in view *Plantago* pollen was examined, since recent Danish work has shown that it is a good indicator of the density of human habitations. Pollen from the Thorsberg bog show *Plantago* pollen becoming scarce at the end of the Bronze Age, just like that of grain.

PLATE I

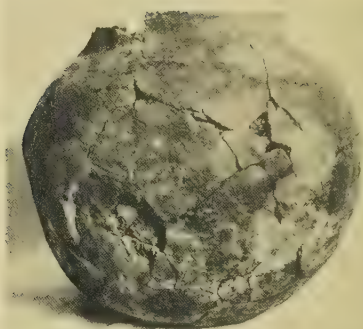


STONE CIRCLE AND STANDING STONE ON SITE OF FORMER BARROW;
PERHAPS THE MEETING-PLACE OF ANGELN (B on the map, fig. 7)

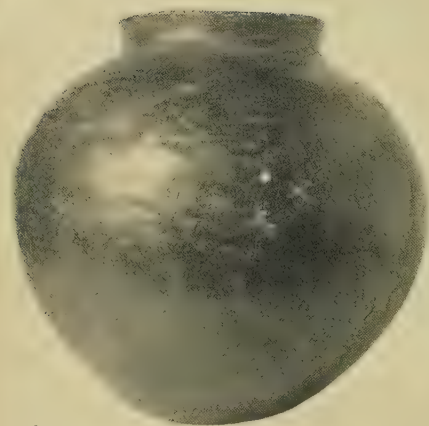
PLATE II



A



B



C



D

CONTINENTAL (left) AND ENGLISH (right) URNS COMPARED

A, C from SÜDERBRARUP (1:4).

B from SUTTON COURTENAY, BERKS.

D from LACKFORD, SUFFOLK

CEMETERIES OF THE 5TH & 6TH CENTURIES

○ - with no urns

● - 1-3 urns

⦿ - more than 3 urns



Denmark, after Brøndsted
Schleswig-Holstein,
after Genrich

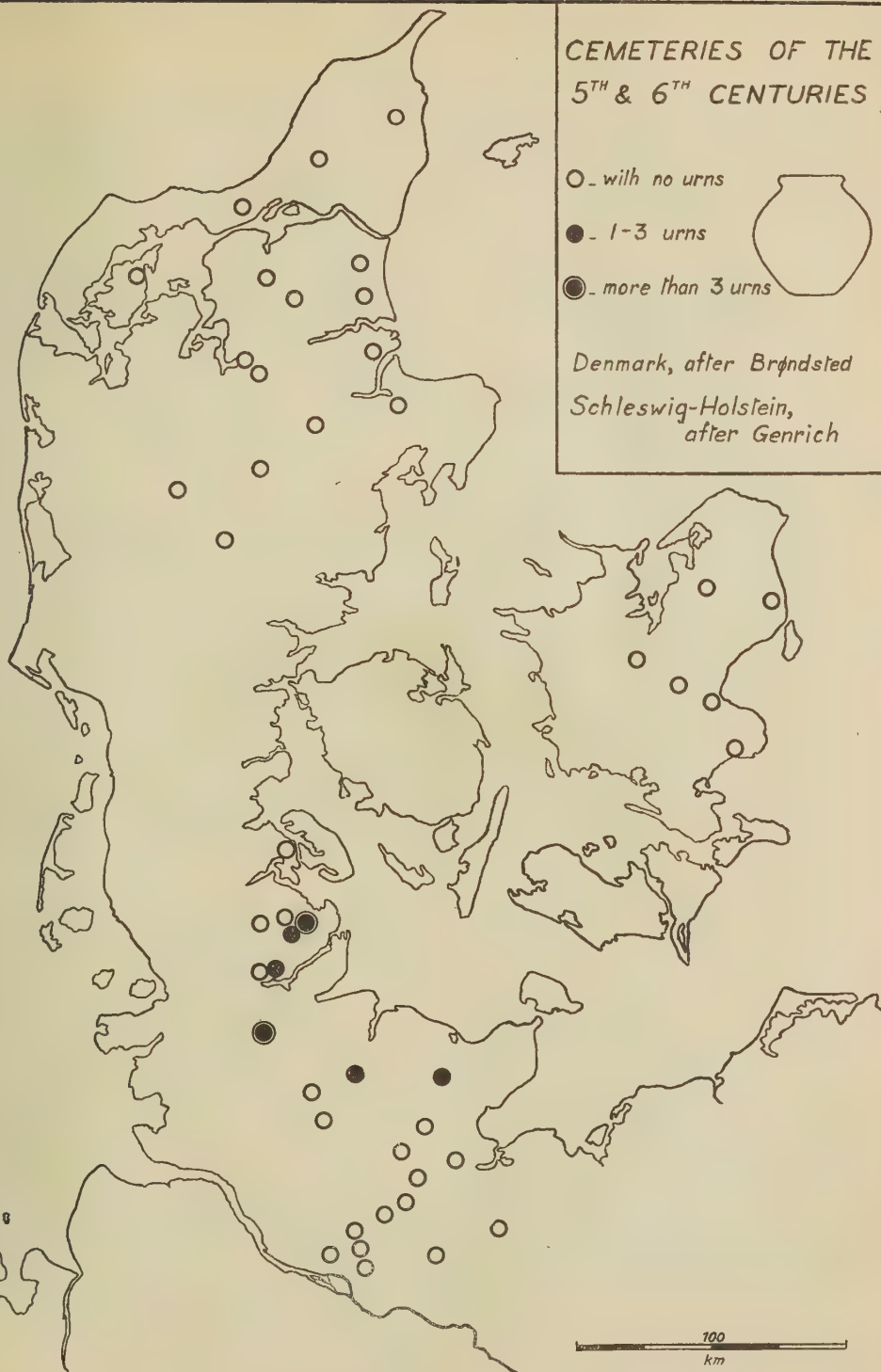


FIG. 6

Archaeology and palaeobotany thus both show that the dense population of Angeln and Schwansen ceased at the beginning of the Iron Age. What happened to the Bronze Age folk is not known; presumably they emigrated elsewhere. Agriculture ceased almost entirely, and the deserted countryside went back to forest. In the 2nd century B.C. a new period of settlement began, the people coming mostly from the lower Elbe region. They settled first on the light sandy soils (FIGS. 2 and 3). About the beginning of the Christian Era a period of internal colonisation began, connected probably with extensive forest clearance (FIG. 4). On a map showing population-density in Schleswig-Holstein during the first centuries of the Christian era (FIG. 5). Angeln would appear as the dense nucleus of a settlement-area extending north and south beyond the frontiers of Angeln, and separated from adjacent regions on the north, south and west by a wide uninhabited belt¹⁵. With the increase in the population went a simultaneous increase in agriculture. On the western frontier of Angeln plentiful deposits of bog-iron ore were found and a large smelting centre grew up. It is reasonable to suppose that this well defined settlement area in south Schleswig, separated from the west by waste land, constituted the territory of a tribe.

During the years 1858-61 a great find was made in the centre of the oldest Iron Age settlement area in Süderbrarup, consisting of weapons, tools, pottery, textiles and ornaments¹⁶. It was considered to be of a sacrificial character, dedicated after battle as part of the booty; thus these big bog-finds would be evidence of tribal battles¹⁷. But in 1935 it was shown that the objects in the Thorsberg find ranged in date over four centuries¹⁸, so that they could not have been deposited on a single occasion, but at intervals, as votive offerings. The period covered lasted up to about A.D. 400, during which the offerings changed¹⁹. First came pots (as elsewhere in Europe), which had contained butter, animal fats and bones, hazelnuts, flax. With the pots have been found animal skins, horns of cattle and querns. Sometimes there were heaps of stones near by on the bog and (rarely) wooden human figures. The pots are the offerings of a community of farmers. At about A.D. 100 a change occurred and the pots were superseded by metal ornaments belonging to clothes and weapons. From A.D. 200 onwards Roman imports appear, and in the 3rd century isolated gold objects, mostly armlets. From the large quantity of offerings it is legitimate to infer that Thorsberg 'served' a large area, probably the whole of Angeln.

The immediate neighbourhood of Süderbrarup abounds in Iron Age sites (FIG. 7). On the high ground round the Thorsberg bog are about twenty barrows, some of the Late Bronze Age, with secondary interments of the Early Iron Age and Roman periods; others were constructed in Viking times. In the middle of the area are two large cemeteries, one near the church and in the market-place having contained more than 1000 urn-burials dating from early Roman times down to the 6th century; 1300 graves have already been found. The other, north of the railway station, consisted of between one and two thousand burials, but nearly all of them were destroyed, only about twenty

¹⁵ F. Tischler, *Fuhlsbüttel, ein Beitrag zur Sachsenfrage*. Neumünster 1937, map 1.

¹⁶ C. Engelhardt, *Thorsbjerg Mosefund*. Copenhagen 1863. H. Jankuhn, *Nydam und Thorsberg*. Neumünster 1950.

¹⁷ J. J. A. Worsaae, *Om Slesvigs eller Sønderjyllands Oldtidsminder*. Copenhagen 1865, p. 52 ff.

¹⁸ H. Jankuhn, 'Zur Deutung des Moorfundes von Thorsberg'. *Forschungen und Fortschritte* 12, 1936, p. 202.

¹⁹ H. Jankuhn, 'Die religionsgeschichtliche Bedeutung des Thorsberger Fundes'. *ibid*, p. 365 ff.

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urns (3rd to 6th century) surviving. East of the sacrificial bog is a holy well to which, according to an old tradition, processions were led in pre-Christian times. Near it have been found several small pits, perhaps hearths, dating from the 1st century A.D. All this is evidence of the outstanding importance of the area in early times. It is to be



FIG. 7

inferred that Thorsberg was the religious, and probably also the political and economic, centre of the region.

In the years 1859-61 an uncommon type of sanctuary was discovered at Nydam in the middle of which were two or three large boats²⁰. Only one of these—the one made

²⁰ C. Engelhardt, *Nydam Mosefund*. Copenhagen 1865.

of oak—was preserved²¹. In and around the boats had been deposited coins, iron weapons and (more rarely) ornaments. The finds cover the period 2nd–3rd to 6th century A.D. Nydam, like Thorsberg, is a sacrificial site, used over a long period, but the finds indicate another kind of rite; in the middle were the boats, and the offerings were mainly weapons.

The settlement of southeastern Schleswig reached its climax in the 3rd and 4th centuries A.D. (FIG. 4). Judging from the number of the cemeteries the land must have been densely populated. From the fact that the belt of waste land in the south and west was encroached upon one may suppose that good soil was getting scarce. In the 5th century the cemeteries decrease in number and in the 6th they cease altogether. Bede says about A.D. 730 that the country of Angulus was deserted. He puts it 'inter provincias Jutarum et Saxonum', a position corresponding to that of the present Angeln. Not until about A.D. 800 do signs of re-occupation appear.

During the 4th and 5th centuries various types of pottery were made in southeastern Schleswig; these, being only occasionally found south of the Eider and north of Flensburg, must be regarded as strictly local types. They are globular with a low rim. Urns of these types are found in the latest (5th and 6th century) cemeteries of Angeln. Corresponding types of urns have been found in English cemeteries (PLATE II), representing the continuation here of those very types which ceased in Angeln²². It has already been shown that cruciform brooches, which are typical of the Anglian region in England, have their oldest prototypes in Schleswig-Holstein²³. The archaeological evidence thus confirms the emigration to Britain which caused the Angeln region to become deserted in Bede's time. Bede knew the facts, and there is all the less reason to distrust his account in the fact that the Norwegian Othere (Ottar) who visited the country in the 9th century knew also of the tradition²⁴.

On the other hand, historical research has recently shown that the distribution of population as recorded by Ptolemy²⁵ is untrustworthy as evidence for a Central European home of the Angles. Nor does archaeology provide any evidence of connection between Central Germany and England during the 5th century. It does however show very conclusively that the dense population of Angeln migrated to England during the 5th and 6th centuries A.D.; so that the hypothesis that the Anglians came from Schleswig is securely based.

²¹ H. Jankuhn, *Nydam und Thorsberg*. Neumünster 1950, p. 27 ff.

²² F. Tischler, 'Bemerkenswerte Funde der Völkerwanderungszeit im Ruhrmündungsraum'. *Probleme der Küstenforschung im südlichen Nordseegebiet* 1. 3, 1, 1942, pp. 84–95. p. 90, fig. 5 and p. 91, fig. 7 a.

²³ A. Plettke, *Ursprung und Ausbreitung der Angeln und Sachsen*. Hildesheim-Leipzig 1921, p. 8 f. A. Genrich, 'Formenkreise und Stammesgruppen in Schleswig-Holstein an Hand der geschlossenen Funde des 3–6. Jahrhunderts'. *Kieler Dissertation*, 1939. H. Jankuhn, 'Zur Frage nach der Urheimat der Angeln'. loc. cit., p. 41 ff.

²⁴ *King Alfreds Orosius*, edited by Henry Sweet, M.A. Part I, London 1883, p. 19.

²⁵ Th. Steche, loc. cit. G. Schütte, *Ptolemy's Map of Northern Europe*, 1917. G. Schütte, 'Anglernes angelske Hjemstavn stadfaestet'. *Aarbøger for nordisk Oldkyndighed og Historie*, 1943, pp. 207–11.

The Date of Caernarvon Castle

by A. J. TAYLOR

(*Inspector of Ancient Monuments for Wales*)

NEARLY forty years ago Sir Charles Peers read a paper of outstanding importance on Caernarvon Castle. The description he then gave of the various buildings of which the castle is composed is printed in the *Transactions of the Cymmrodorion Society*¹. It will never be bettered, combining as it does a mastery of detail with a simple and lucid presentation of a most complicated structure. It is prefaced by an analysis of contemporary documents bearing on the original work of construction, on the gradual progress of building and on the castle's subsequent history². On the strength of these records a concise building sequence is established³, and this has in its turn been adopted as the basis of all subsequent writings on the castle⁴. A dated ground-plan, setting out this sequence in graphic form, accompanied the printed account, and has since been widely reproduced.

The purpose of the present review is to examine afresh certain of the early documents on which this currently accepted dating is based and to reconsider, where this seems desirable, the interpretations of them previously put forward. If, as will be suggested, some of these interpretations require modification, a major revision in the dating sequence will necessarily follow.

Attention has lately been given elsewhere to the earliest stages of building work at Caernarvon Castle, following the arrival of the English forces there in the spring of 1283⁵, and it is not proposed to consider here how soon the initial timber structures then erected began to be superseded by buildings of stone⁶. Let us, instead, first examine the important memorandum which was added as a postscript to a letter sent by Walter

¹ Session 1915-16 (henceforward cited as *T.C.S.*), pp. 28-69.

² *ibid.*, pp. 1-28.

³ In short, this is : S. and W. section 1285-91 ; S. and E. section 1295-1301 ; N. section 1315-22.

⁴ e.g. Caernarvon Castle *Official Guide*, which has passed through two editions and many printings, and Sidney Toy's *Castles* (1939), p. 195.

⁵ See the present writer's paper in *History*, Oct. 1950, 'The Birth of Edward of Caernarvon and the Beginnings of Caernarvon Castle' (Historical Revision, No. cxvi).

⁶ The point is discussed on pp. 45-6 of Dr J. G. Edwards's Rhys Memorial Lecture, 'Edward I's Castle-building in Wales' (*Proc. Brit. Acad.*, xxxii, pp. 15-81), which appeared after this paper was written. Dr Edwards shows that, if due allowance is made for certain missing rolls of accounts, there is room for supposing that the stonework of the castle may well have proceeded much more rapidly than has hitherto been realized. That this was indeed the case is suggested by the fact that already in the third building season (1285), when Peers thought the main castle construction had scarcely begun, lead was being sent by sea from Bristol for roofing the castle buildings : in the Liberate Roll of 14 Edward I there is note of the payment of £31 7s 8d to Peter de la Mare, constable of Bristol castle, for 14 loads of lead bought and sent by him *usque castrum nostrum de Kaerenarvon ad domus nostras eiusdem castri inde cooperiendas anno regni nostri terciodecimo* (*P.R.O.*, Liberate Roll, no. 62).

of Hereford and Hugh of Leominster, master mason and chamberlain of Caernarvon respectively, to the treasurer and barons of the Exchequer on 25 February 1296. The interpretation of this short record is crucial in no less than three particulars, and it is necessary to consider it in the original⁷:

Memorandum quod in muro inchoato circa motam castri de Carnaruan sunt iiij turres inchoate, qui quidem murus continet in longitudine xvij perticatas; et de istis perticatis viij perticate continent in altitudine xij pedes, et x perticate continent in altitudine xxiiij pedes; et iste murus continet in spissitudine xv pedes.

First, to what period of time does the word *inchoato* (begun, in the sense of unfinished) refer? Peers thought it meant work started in 1295 and carried to the stage described in the officials' letter by February 1296⁸. Another interpretation is, however, possible. As Peers himself rightly says, the earlier part of the letter shows that the summer months of 1295 were wholly given up to the task of rebuilding the partly destroyed town wall; that this took until 10 September; and that 'they then took up work on the castle till 27 November, *when building stopped for the winter* (my italics), and the workmen were employed on dressing freestone, working timbers, quarrying and carrying stone from the quarries, and forging the iron work'⁹. In other words there were, in 1295, only 2½ autumn months when the work carried out was mainly actual construction work on the castle. Even if we assume the necessary amount of stone for this had already been quarried and brought during the summer months, it seems at least doubtful whether this short period, perhaps only part of it good building weather, could have seen the erection of a wall 15 feet thick, an average of 18 feet high and (accepting for the moment Peers's interpretation of the figures)¹⁰ 150 yards long, and incorporating the bases of four towers, presumably built to something like the same average height. It seems still less credible in view of the priority which the surviving accounts suggest was being given in the 1295 season to the labour requirements of the new castle rising at Beaumaris¹¹. If these doubts are valid, it looks as if the *muris inchoatus* and the *quatuor turres inchoate* may actually have been begun before the disaster of October 1294¹²—which probably means before the end of 1287, the last previous year in which a substantial sum had been spent on the Caernarvon works¹³. In this case, the purpose of their mention in the schedule to the letter of February 1296, would be to set down a record of the point previously reached on the section of the castle on which it was intended to resume work at the commencement of the coming season. One cannot help wondering whether, if this section had really only been begun in the preceding September, (a) it would have been necessary to write such a note about it at all, and (b), if it was necessary, whether we might not have expected some such words as *in muro nuper incepto* rather than the vaguer *muro inchoato*. Possibly the figures were given in answer to an actual question

⁷ P.R.O., *Exchequer Accounts*, 5/18, no. 11. The whole document has now been printed by J. G. Edwards, loc. cit., pp. 79–80; its implications are examined *ibid.*, pp. 46–7.

⁸ T.C.S., p. 12: 'The new work was evidently started in 1295, and by the end of the year had progressed as already described'.

⁹ *ibid.*, p. 10.

¹⁰ *ibid.*, p. 12, note 1.

¹¹ Works expenses at Beaumaris were £8555 7s 10d for one year (18 April 1295–7 May 1296) (Pipe Roll, no. 158), as compared with £4393 13s 7½d at Caernarvon for four years (11 Sept. 1295–28 Sept. 1299) (*ibid.*, no. 146, m. 24).

¹² J. E. Morris, *Welsh Wars of Edward the First*, pp. 242, 253.

¹³ *Pipe Roll*, no. 136.

THE DATE OF CAERNARVON CASTLE

from the Exchequer, who may have required an *aide memoire* as to what remained to be done to complete this part of the building; in the form in which they are stated they would provide an indication of the scale of the work to be undertaken, as well as of the extent of that previously done.

What of the phrase *circa motam castrī*? Peers rendered it 'round the motte of the castle'¹⁴, and on the strength of this identified the building referred to with the southeast side of the castle, including the Northeast Tower, the Queen's Gate and the Black Tower.

It cannot be questioned that there was formerly a motte in the upper ward or outer bailey of the castle, dating from the late 11th century and largely surviving until after 1870 (PLATE IVb¹⁵). But it is to be doubted whether *circa motam castrī* means 'round the castle motte' or whether it would be written by a 13th-century clerk who wanted to say that; if he had used such a phrase at all he would have said simply *circa motam*, the addition of *castrī* being in this context as tautological in Latin as that of the words 'of the castle' in English. The true meaning of *circa motam castrī* is, it is believed, something quite different. A clue to it is provided by a phrase in the nearly contemporary accounts for the works at Beaumaris, recording the payment of £1468 12s 0d wages *fossatorum faciencium fossata et motam ubi castrum de Bello Marisco situm est*, from April to September 1295¹⁶. At no time was there a motte at Beaumaris, and we may be sure the diggers of 1295 were not employed on building one. The *fossata* they dug were the foundation trenches for the towers and curtain walls, and the *mota* was the castle ditch or moat. And so it was at Caernarvon¹⁷. When this is the meaning of *mota*, *mota castrī* is not tautological, for where there is a defended borough it may be used in contradistinction, expressed or implied, to *mota ville*. The *muris inchoatus circa motam castrī* is thus not 'the wall begun around the motte', but the 'unfinished wall along the castle ditch'. It is, in other words, the wall forming the north side of the castle, and the four *turres inchoate* are the Well Tower, the Granary Tower and the towers flanking the King's Gate. It has always been a difficulty with the old theory that the section of wall to which Peers thought the description referred contains six towers and not four and that at best it extended only halfway round the motte.

Another, more serious, objection arises from the measurements. Being thus led to identify the *muris inchoatus* and its four towers as the section round the east end of the motte from the Northeast Tower to the angle west of the Black Tower, Peers says that 'its length of 18 perches may be taken as 450 feet; the perch in 1316 being said to contain 25 feet. Measuring round the faces of the towers and the walls, on the outside, from the Northeast Tower to the bend in the south curtain, this figure is approximately correct'¹⁸. This means, however, that in order to arrive at a figure which for the purposes of the required identification is only approximately correct, an abnormal and exceptional reckoning of the linear perch has to be adopted, the standard perch being one of 5½ yards, i.e. 16 feet 6 inches, not 25 feet. Certainly there is, as Peers says, a reference to a perch of 25 feet at Caernarvon in an account for work done on the quay there in

¹⁴ *T.C.S.*, pp. 11, 12.

¹⁵ I have to thank Mr W. J. Hemp for calling my attention to this photograph and allowing me to reproduce it here. Plates II and IVa are from photographs by Mr G. B. Mason of the National Buildings Record, to whom thanks are due for permitting their reproduction. Plates I and III are from photographs by the author. The plan (p. 31) has been redrawn from a survey in the possession of the Ministry of Works.

¹⁶ Pipe Roll, no. 158.

¹⁷ Indeed the word is actually used of the Caernarvon castle ditch in a record of June 1283: '... pro fossur' et apparatu noue mote de Caernaruan ...' (*Exch. Accts.*, 351/9).

¹⁸ *T.C.S.*, p. 12, note 1.

November 1316¹⁹. But the fact that in this instance the clerk goes out of his way to define a length for the perch at all suggests that it was not the ordinary reckoning, which would surely have needed no such definition²⁰. There would seem to be little justification for applying it, without considerable reserve, to the memorandum of 1296. On the contrary, the absence of a qualifying figure in that document leads one to suppose that the 18 perches of the *murus inchoatus* were standard perches, giving a length not of 450 but of 297 feet.

If we look carefully at the wording of our record, we see that the measurements of length, height and breadth which it gives relate only to the *murus* and not to the *turres*. There is thus a double error in an interpretation which besides increasing what is likely to be the true length by 153 feet accounts for that increase by 'measuring round the faces of the towers and the walls'; and the fact that the result is 'approximately correct' can only be regarded as a coincidence. Let us now, therefore, omit the towers and take instead strict measurements of the lengths of wall bordering the castle ditch, and see what light they throw on our view that this, and not the wall surrounding the south and east sides of the site of the motte, is the *murus inchoatus circa motam castris* . . . *qui* . . . *continet in longitudine xviii perticatas*. Each section of wall is measured between the points at which its outer face is cut by the outer face of a contiguous tower, the measurements being given to the nearest six inches. Taking the Eagle Tower as our starting point, we have a length of 119 feet from there to the west side of the Well Tower; from the well-projection on the east side of the Well Tower to the west face of the King's Gate the length is 69 feet 6 inches; from the east face of the King's Gate to the west side of the Granary Tower is 55 feet; and from the east face of the Granary Tower to the Northeast Tower is a further 61 feet 6 inches. This gives a total wall length of 305 feet, which tallies closely enough with the 297 feet represented by a wall of 18 perches of 5½ yards each. The slight discrepancy may arise from the fact that, inwards from both the Eagle and the Northeast towers, a little curtain walling had already been built in continuation of those structures (PLATES IIA and IIIA), to a greater height in the former case than in the latter. Beside the Eagle Tower this return wall terminated on a sloping line, and we cannot now be sure at exactly what level the 1296 measurements were taken. But the figures are, it is suggested, sufficiently close to afford conclusive evidence that the wall *circa motam castris* is that fronting the castle ditch and linking the two ends of the town wall²¹.

We may now consider the supposed change of build marked by the shift in the line of the south curtain midway between the Black Tower and the Chamberlain Tower. According to Sir Charles Peers, this turn in the wall represents a break between work built before 1291 (to the west) and work built after 1295 (to the east)²². Nevertheless it

¹⁹ *Exch. Accts.*, 486/29: 'Henrico de Elreton cementario pro duodecim perticatis Kaye ville de Kaern' ad tascam faciendis de petra et calce sumptibus suis propriis, percipienti pro perticata continente xxv pedes viii.li. . . . in partem solucionis iiij.xxxvj.li. quas percipere debet pro tasca predicta—lx.li.' Also 'Ricardo de Wykombe pro quinque perticatis, dimidio rod', tribus pedibus Kaye predicte cum arena inter murum ville et dictam kayam obstupandis, qualibet perticata continente xxv pedes . . . xl.s.'

²⁰ Possibly a long perch was allowed because a water wall presented additional difficulties. Dr. J. G. Edwards draws my attention to the fact that calculations based on payments made in July 1279 for building the side of the castle ditch at Flint (*Chancery Miscellanea*, 2/2/17) point to a perch of 25 feet. This was probably a wet ditch and similar considerations may have applied.

²¹ It is not practicable to base any arguments on the heights to which the wall is described as standing in 1296, as the depth of the foundation from which these were presumably taken is unknown.

²² *T.C.S.*, pp. 13, 59.



CAERNARVON CASTLE: THE SOUTHERN FAÇADE FROM ACROSS THE RIVER SEIONT

Ph. A. J. Taylor

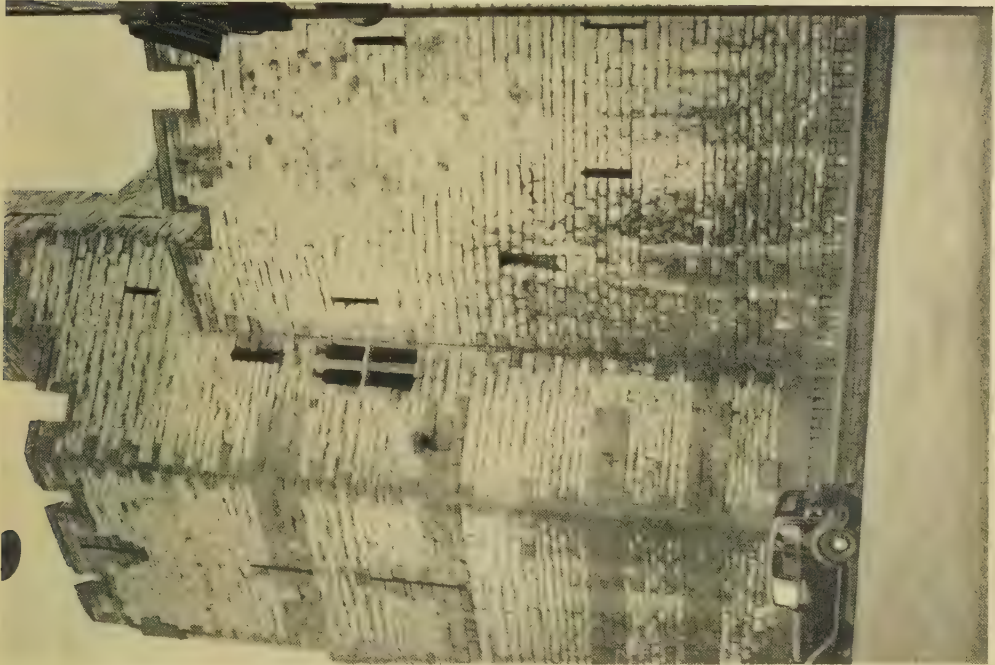


a. CAERNARVON CASTLE: JUNCTION IN CURTAIN WALL EAST OF
EAGLE TOWER (NORTH FACE OF WALL.)



b. CAERNARVON CASTLE: JUNCTION IN CURTAIN WALL EAST OF
EAGLE TOWER (SOUTH FACE OF WALL.)

PLATE III



a. CAERNARYON CASTLE; JUNCTION IN CURTAIN WALL
WEST OF NORTHEAST TOWER

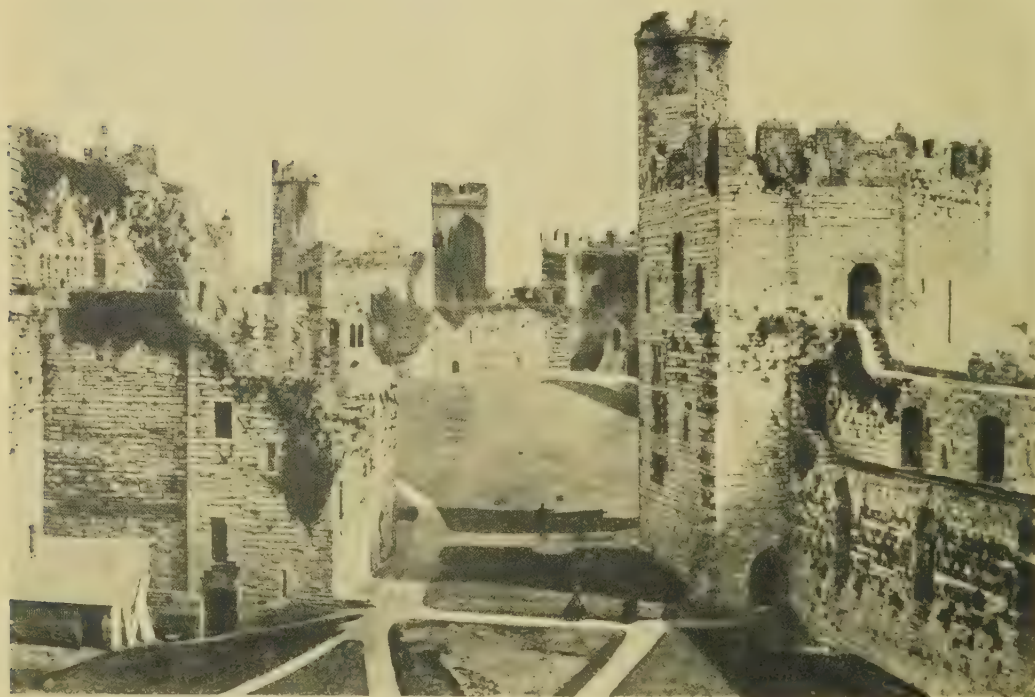


b. CAERNARYON CASTLE; SOUTH CURTAIN WALL BETWEEN
BLACK AND CHAMBERLAIN TOWERS

Pls. A. J. Taylor



a. CAERNARVON CASTLE: VAULTING OVER PASSAGE OF KING'S GATE
Ph. G. B. Mason. Copyright, National Buildings Record



b. CAERNARVON CASTLE: VIEW TAKEN c. 1870. SHOWING MOTTE IN UPPER WARD

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is impossible not to be struck by the contrast between the form of this 'break' and that of the marked changes of build, already briefly referred to, which occur at either end of the north front. At the west end, a few feet from the Eagle Tower, the external decorative bands of Aberpwl stone terminate, and the regular coursing of the facework, continued evenly thus far from tower to curtain, is interrupted throughout its height (PLATE IIA). The change at this point is no less noticeable on the inner side of the wall (PLATE IIB). At the east end, beside the Northeast Tower, there is another break characterized by similar features. A little to the west of the tower there is a slight angle in the wall, the break at ground level coming immediately west of this; the sloping apron at the base of the wall, which is continuous round the east end of the castle, here stops abruptly, while a little higher up there is an equally abrupt finish, on the line of the quoin, to the lowest of the three-course Aberpwl bands (PLATE IIIA). There can be no doubt that these irregularities, both east of the Eagle Tower and west of the Northeast Tower, reflect an appreciable pause in the building operations, and it is clear from the way the masonry slopes that the north wall was completed against masonry already existing to the east and west of it, and not vice-versa. When, however, we come to look at the change of alignment between the Black Tower and the Chamberlain Tower we are confronted with something wholly different. On either side the facework is alike; the decorative banding is continued regularly (PLATE I); internally there is no break or step in the level of the wall passages, such as we might have expected if the changed alignment masked a 'join-up' of two different building periods; and, lastly, when we examine the foot of the wall on the outside we find that its seven lowest courses follow a perfectly straight alignment from tower to tower (PLATE IIIB). It therefore seems to be open to question whether the change of direction taken by the wall above this level affords any evidence whatever of differing dates of construction. If it does not, there must be an alternative explanation, and it is not far to seek. The wall at this point may be presumed to be built on the site of the ditch of the former motte. The sloping apron which begins here (PLATE IIIB) and reinforces the base of the curtain all round the east end of the castle was introduced to revet and contain ground, the stability of which was weakened by its artificial nature. The surviving short foundation, which can be seen within the castle pointing across the upper ward in the direction of the King's Gate, shows that the Edwardian builders intended to complete this reinforcement by circling the inner as well as the outer side of the mound with a containing wall²³. It looks very much as if the twist in the south curtain relates only to the existence of the former ditch round the motte and to the planned junction with this future cross wall. It appears to provide no evidence which can properly be regarded as indicating a halt and subsequent resumption of building.

This is a convenient point at which to summarize the argument and note its implications. We have suggested

- (i) that the wall described as *inchoatus* in the schedule of February 1296 is unlikely to have been constructed to the stated dimensions in the short period available in 1295, and must in all probability have been commenced at a date not later than 1287;
- (ii) that the wall in question is that forming the north side of the castle, containing the Well Tower, the twin towers of the King's Gate and the Granary Tower; and not, as hitherto held, the wall forming the east and southeast side of the castle, containing the Black Tower, the Queen's Gate and the Northeast Tower;

²³ The revetting suggests the possibility that the original intention may have been not to remove the motte, but rather to put a heavy building on it. A tower in this position would have been the crown of the whole design.

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- (iii) that the measurements given in the schedule represent standard perches of $5\frac{1}{2}$ yards, and that on this basis 165 feet of the north curtain had been built to a height of 24 feet from the foundation, and the remainder to a height of 12 feet, by 1287, or at latest by 1292, when the first series of works payments cease.
- (iv) that there is no essential difference in building date between the sections of the south curtain to east and west of the change of alignment between the Black Tower and the Chamberlain Tower ;
- (v) that there are thus only two, not three, constructional breaks in the building, viz. to the east of the Eagle Tower and to the west of the Northeast Tower, the lowest portion of the intervening curtain being contemporaneous, to a height of at least 12 feet from the foundation, with the rest of the enceinte.

Taken in relation to each other, these considerations imply :

- (i) that the whole of the castle outside the ends of the town wall, i.e. from and including the Eagle Tower, round the south side as far as and including the Northeast Tower, had reached an advanced stage by 1292²⁴ ; for the two ends of this section are demonstrably not later than the intervening north curtain, which, if our interpretation is correct, had itself been built to a height of from 12 to 24 feet by that year ;
- (ii) that the conception of the building as falling into three sharply defined periods, viz. south and west section 1285-91, south and east section 1295-1301 and north section 1315²⁵-22, cannot be sustained ;
- (iii) that the castle is on the contrary a single design conceived as a unity at the time work was begun on it in 1283-4, and, except on the north side, carried a considerable way towards completion within the succeeding three or four years ;
- (iv) that the growth after 1296 was essentially an 'upward' growth, with the gradual addition of height to work already well started in varying degree round the whole circuit of the castle, and not, as has hitherto been held, an 'outward' growth, with the successive addition of entirely new sections to a first block which had consisted only of the lower storeys of the Eagle Tower and the buildings immediately to the south and southeast of it²⁶ ; this 'upward' growth was most marked on the north side of the castle, where prior to 1292 building had not exceeded a height of 24 feet above moat level.

The above propositions are embodied in the accompanying ground plan (opposite).

²⁴ The three lower storeys of the Eagle Tower, which are earlier than the fourth storey and surmounting turrets (*T.C.S.*, p. 46), may represent the general height reached by the south curtains and buildings ; the fact that the hall had been built to gable height before the time of Madoc's revolt (cf. *ibid.*, p. 36) shows there must have been at least equivalent height in the section of the curtain against which it was built. The uppermost part of the curtain between the Eagle and the Queen's towers, the beginning of which is marked by a string course, was probably added when the Eagle Tower was heightened, i.e. after 1295.

²⁵ There is in any case no particular virtue in this date. Work was certainly going on in 1304-5 (see below, p. 33). I have shown elsewhere (*Trans. Caernarvonshire Hist. Soc.*, 1948, pp. 16 ff.) that it was also in progress in 1309, and that it was in that year, not 1315, that Walter of Hereford died and his deputy Henry of Ellerton succeeded him as master mason at Caernarvon.

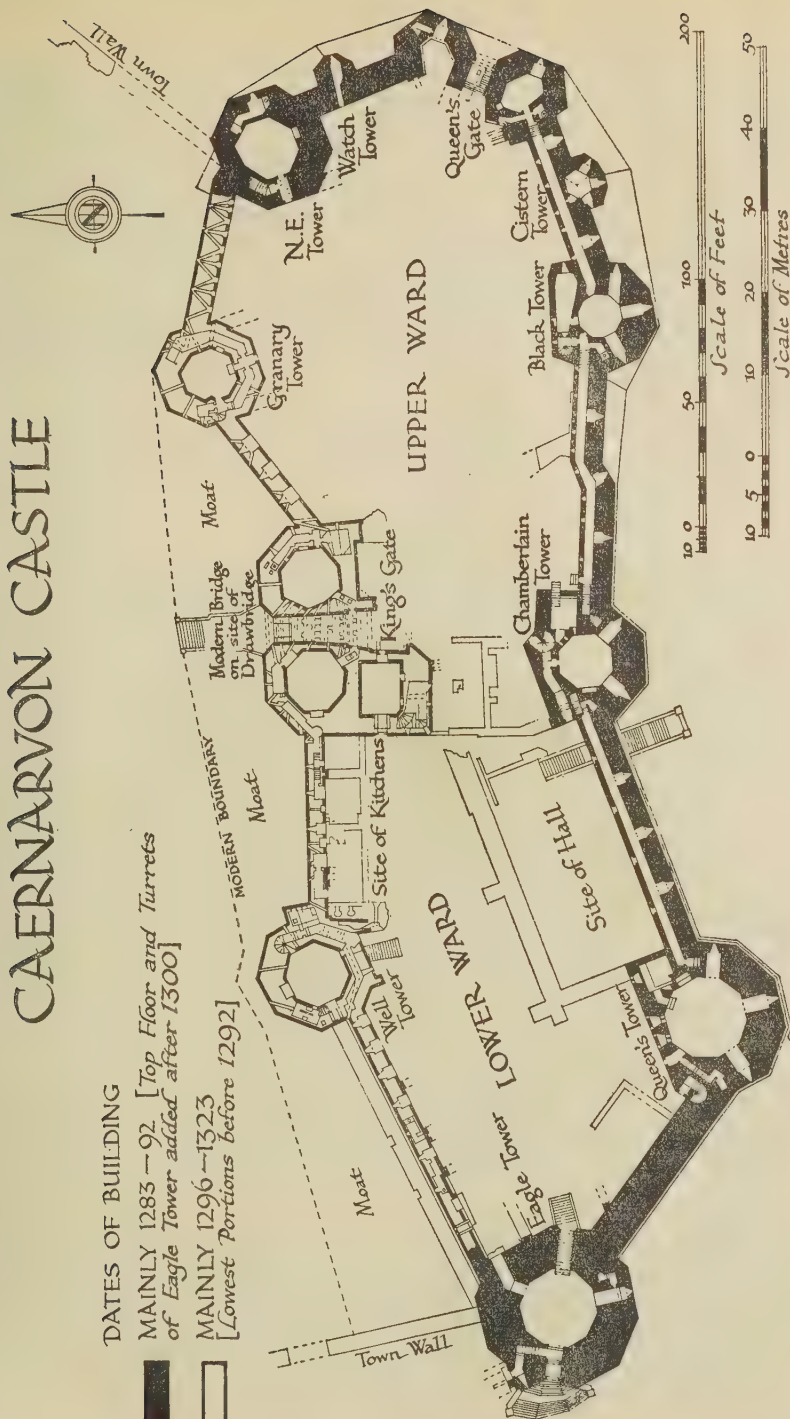
²⁶ This reading of the building is at any rate partially supported by documentary evidence for the completion of the heightening of the Eagle Tower and another tower in about 1316, for the erection of the statue over the King's Gate in 1320, and for work on the hall over that gate in 1321-2 (*T.C.S.*, pp. 16-18).

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DATES OF BUILDING

MAINLY 1283—92. [Top Floor and Turrets
of Eagle Tower added after 1300]

MAINLY 1296—1323
[Lowest Portions before 1292]



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It is advisable that we should consider this theory of the building development of the castle in relation to such works accounts as are preserved on the Pipe Rolls²⁷. Our view, based as it is on what seems to be the evidence of the buildings themselves and on the note of February 1296, would of course be unacceptable if it were found to be incompatible with the expenditure there recorded. While it is difficult to make reliable comparisons, and easy perhaps to draw parallels that seem closer than they may really be, with contemporary works at other castles, we must nevertheless make the attempt if the Pipe Roll accounts are to throw any light on our problem. Let us, therefore, examine the figures for Conway and Harlech, and compare the achievement which they can be seen to represent with the corresponding figure and suggested achievement at Caernarvon. The total 'works' and related expenditure given in the Pipe Rolls for the three places is as follows:

<i>Caernarvon</i>	£6566 3s 0½d.	(Jan. 1283–Oct. 1290 ²⁸ ; mostly before Sept. 1287).
<i>Conway</i>	£7714 11s 10½d.	(Nov. 1284–Sept. 1289 ²⁸ ; mostly before Oct. 1287).
<i>Harlech</i>	£8178 5s 7d.	(May 1285–Dec. 1290 ²⁸ ; mostly before Dec. 1289).

According to the hitherto accepted dating, the only part of Caernarvon castle built during the period covered by the first of these figures (which it must be remembered includes also the cost of the town wall and of work on the quay) is from the Eagle Tower southwards to a point east of the Chamberlain Tower. The question we now have to ask ourselves is whether it is reasonable to suppose that the figure could also have included the cost of the unfinished Queen's Gate with the buildings on either side of it from the point east of the Chamberlain Tower to the break west of the Northeast Tower, together with the lower 12 to 24 feet of the northern towers and curtains. At first sight it may seem improbable that so much could be done for £1150 less than it cost to build Conway and nearly £2000 less than it cost to build Harlech. But this is not so. The Conway figure covers virtually the whole of that great castle as we now see it, together with a town wall more than twice the length of that of Caernarvon and originally containing 24 towers and 3 twin-towered gateways, as compared with the Caernarvon wall's 8 towers and 2 gateways. The greater cost of Harlech (which of course also covers the whole castle except the Water Gate and the outer enclosure of the castle rock²⁹) is to be accounted for by the fact that, being 'concentric' in plan, it has an additional ring of towers and curtains which is absent at Conway and Caernarvon; by the great size of its three-storey keep-gatehouse; by the expense of digging its wide rock-cut ditches; and by the relatively higher charges for carriage and materials arising from its more remote situation. If these factors, and particularly the imposing scale of the completed works at Conway and Harlech, are taken into consideration, it does not seem that our interpretation of the building sequence at Caernarvon places an undue strain on the Pipe Roll figures for the 1280s. With regard to the figure of £4393 13s 7½d recorded as having been spent on

²⁷ *Pipe Rolls*, nos. 131, m. 26d, and 136, m. 28.

²⁸ The dates are those of accounting periods within which payments were made. There is evidence that at Conway and Harlech works were in progress earlier than the commencing date of the account, while at Caernarvon the figure here given actually only covers the work from August 1284 onwards although it had then been in progress for more than a year.

²⁹ Cf. the present writer's note, 'Harlech Castle: the dating of the outer enclosure', in *Journal of the Merioneth Hist. and Record Soc.*, 1951, pp. 202–3.

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works at the castle from September 1295 to September 1299³⁰, this must be presumed to have included not only much work on raising the *murus inchoatus* and its towers and elaborate gate (PLATE IVa), but also most of the rebuilding and refitting of the pre-1290 structures which had been sacked by the Welsh in the winter of 1294-5.

Finally, it is to be noted, the statement on p. 6 of Peers's article that expenditure of £582 12s 8½d was incurred on the castle in 1288-9 requires correction, in that the account in question dates from 1304-5³¹. This is important for two reasons. It shows that work was going forward fairly steadily between 1301 and 1315, years which are a blank period in Peers's scheme of dating³². Secondly, the true dating of this account removes the only evidence that had ever been produced for naming Walter of Hereford as master mason at Caernarvon prior to 1295. There is in fact no documentary record to connect this well-known English builder with the Caernarvon works until 30 June of that year³³. There is, on the other hand, precise evidence to show that he was master of the works at Vale Royal abbey in 1287, and also that he still held that post and received allowances *pro robis* in respect of it during the year ending 19 November 1290³⁴. It would perhaps not have been altogether impossible for Walter of Hereford to be master mason at two such important works as Vale Royal and Caernarvon simultaneously, but the obvious difficulties of such an arrangement appear on the whole to make it unlikely. It seems unlikely, too, that the Wardrobe Book of 18 Edward I would have referred to Master Walter simply and solely as 'cementarius, magister operis cementarie ecclesie Abbath' de Valle Regali³⁵ if he were in fact at that time already employed as the master of the works at Caernarvon. The evidence is not as explicit as we could wish, but the weight of it seems to be against Walter's being connected with Caernarvon during the creative and formative years between 1283 and 1292 when, as we believe, the foundations of the *whole* castle were laid and the entire southern façade carried a long way towards completion. If, then, we eliminate Walter of Hereford from being concerned with the building before 1295, the records name only one other to whom the architectural creation of the castle can be attributed. This is Master James of St. George, the *magister operacionum regis in Wallia*.

In a reference to this problem published in 1950, but written in 1947 when the author was unaware of the Vale Royal evidence of 1287 and 1290, the suggestion was made that Walter of Hereford might have been responsible for the essentials of the design of the castle; for Caernarvon's octagonal towers give it an appearance which contrasts in marked degree with that of other castles built under James of St. George's direction, and not least with that of the town wall of Caernarvon itself³⁶. But it is perhaps a poor tribute to a great architect to assume that all his elevations must needs have followed similar patterns. That St. George was a versatile designer appears from the treatment of the Great Tower at Flint, a structure to which a true English parallel has yet to be found. It may well be that the real explanation of Caernarvon's distinctive characteristics is not that they reflect the mind of a different building master, but that they are a deliberate and conscious adornment, introduced at the royal will, of a building intended from its

³⁰ *Pipe Roll*, no. 146.

³¹ The error arose from a wrong dating in the printed *P.R.O. List of Exchequer Accounts, Various* (1912), where the roll (486/1) was given as 16-17 Edward I; in the Search Room copies this has been corrected to 32-33 Edward I. Cf. Edwards, loc. cit., p. 49, note 2.

³² Cf. note 25 above.

³³ *Calendar of Chancery Warrants*, I, p. 63.

³⁴ See my note on 'The Cloister of Vale Royal Abbey' in *Journal of the Chester and North Wales Archaeological, Architectural and Historical Society*, 1949, pp. 295 ff.

³⁵ *ibid*, note 4.

³⁶ *English Historical Review*, LXV, pp. 441-2.

inception to become the principal seat of English power in North Wales and the capital of the new administration promulgated at Rhuddlan in 1284. Moreover, whatever may be the truth of some of the traditions about the early days of the young prince Edward, traditions which cannot now be traced further back than David Powel's 16th-century *Historie of Cambria*³⁷, there is at least some contemporary evidence that Edward I was not insensitive to the symbolic importance of the Gaer in Arvon or uninformed about survivals there of a long-past history³⁸. Such considerations may have helped to influence the king to make the castle of Caernarvon an even more splendid and regal building than the others. Had events turned out differently it might have been destined to become the official palace and residence in Wales of a line of English princes, and in 1284 such a possibility was perhaps not without a place in the king's thought. But when we look beyond the superficial variations and more lavish detail which set the castle apart from its contemporaries, we find many features which have close parallels in the 'round tower' castles. Thus in essentials the King's Gate would, when completed, have been just as much a building of 'keep-gatehouse' type as those, for example, at Harlech or Beaumaris. The wall passages, with their flat shouldered vaults, are almost duplicated by those of Beaumaris, where we know James of St. George directed the works. Or again, the turrets which rise above the roofs of the towers inevitably recall those of Conway and Harlech, and similar turrets were intended at Beaumaris. There is close resemblance also between the pointed barrel vaults of the smaller Caernarvon rooms and those found in the other castles. It does not seem improbable, therefore, that the principal architect may have been the same man as directed the building of the rest of the castles in the group, but that in the case of Caernarvon the terms of his commission required a design that would be out of the ordinary.

Such an explanation would be consistent with such certain knowledge as we have of Master James's and Master Walter's appointments and movements. James of St. George, the senior in rank, was first and foremost a military architect and is named in that capacity as master of the king's works in Wales, including Caernarvon, in the vital period from 1284 onwards. Walter of Hereford's experience appears, on the other hand, to have been primarily ecclesiastical, with work at Winchcombe and Vale Royal abbeys occupying him up till at least 1290, and first coming to take charge at Caernarvon in 1295. If, as we argue, the whole design of Caernarvon had by then been conceived and work had proceeded to different stages over the whole area of the castle, there is nothing unlikely about fetching in the church-builder to execute the repairs and carry on with the work already planned, while the master castle-builder is engaged at full power on the creation of the brand new fortress at Beaumaris. It is more difficult to explain the absence of the master of the king's works from Caernarvon after 1295 if, as the old theory of development proposed, whole sections of the castle still remained to be erected from the ground.

³⁷ Hilda Johnstone, *Edward of Caernarvon* (Manchester U.P., 1946), p. 7, note 1. The survival of the Caernarvon-born prince and the death of his elder brother barely four months later must be regarded as fortuitous; but one is inclined to wonder whether tradition may not preserve a genuine memory of a policy, conceived with the passing of the young Alphonso, of making capital out of the fact that the eldest surviving son was born in this ancient heart of Gwynedd.

³⁸ *Flores Historiarum* (Rolls Series), III, p. 59: (s.a. 1283) *Apud Kaernervan, corpus maximi principis, patris imperatoris nobilis Constantini, erat inventum, et rege iubente in ecclesia honorifice collocatum.* As the late Dr Charlesworth noted (*The Lost Province*, p. 28, note 2), Nennius had already recorded the existence at Segontium of an inscribed tomb of Constantine. Cf. also *Mabinogion* (Everyman edn.), p. 86, and C. E. Stevens in *Archaeological Journal*, xcvi, p. 134.

Radiocarbon dating results from the Old World

by C. B. M. MCBURNEY

(*King's College, Cambridge*)

THE latest complete list of radiocarbon readings received from America brings the total number of dated sites and objects to upwards of 160. Of these 120 are from localities in the New World, and their archaeological implications have been reviewed by G. H. S. Bushnell in the last number of *ANTIQUITY*; the object of the present article is to do the same for the 40 odd results available from the Old World.

One of the most interesting features of these Old World results is the opportunity they offer to make a direct comparison between particular carbon readings and dates fixed by historical deduction, or ultimately based on Baron de Geer's system of varve chronology for the Post-Glacial period. Among the more practical issues of importance to archaeologists that we may hope to see clarified in this way is a closer estimate of the relative accuracy of different kinds of organic materials, and the exact contribution of such sources of contamination as are likely to be met with in the field.

As yet progress towards these goals is limited, though some of the more general features in the situation are already beginning to appear fairly clearly. At the same time it will be seen that a number of important questions still remain unanswered.

The position can perhaps be most conveniently appreciated in a graphical presentation such as that provided by FIG. 1. The horizontal coordinates are those of dates predicted on historical or other general grounds, with a rough estimate of the possible margin of error shown by the length of the horizontal line attached to the symbol. The corresponding vertical lines show the position and margin of error of the carbon dates. This margin of error is that referred to in the previous article, indicating that there is a three-to-one chance of the true value of the sample measured falling within it; the chances of the true value falling within twice that margin, it may be recalled, are about twenty to one.

The outstanding results suggested by this analysis would seem to be the following.

First as regards materials, the superiority of charcoal over most other materials (as predicted by the physicists) is becoming increasingly apparent. Moreover apart from the order of magnitude, there is a distinct suggestion that the type of error noted in readings based on wood or charcoal differs from that offered by materials such as peat or plant-bearing muds. Whereas in the case of peat it begins to look as if the chances of a date so fixed falling above rather than below the true value are about equal, with charcoal and wood all the important errors seem to fall below. Since it is understood that there is no physical or chemical reason why charcoal and wood should provide a later reading than any other substance, it would seem to follow that here the main source of error is in fact provided by contamination with more recent organic substances. During the past season the writer has been engaged in collecting carbon samples from prehistoric sites in Libya, and was impressed by the frequency with which fine hair-like rootlets of living vegetation are found in charcoal layers, particularly in caves and rock-shelters. These rootlets are often found a most surprising distance from the nearest living plant, and have an astonishing capacity to penetrate through rock-like layers of stalagmite. Apart from the percentage of new living matter thus introduced into the ancient samples, it is reasonable to assume that the presence of the rootlets promotes other forms of biological activity ultimately drawing their carbon from the surrounding atmosphere. On the other hand it is also true that *occasionally*, under favourable conditions, deposits—

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particularly those relatively poor in organic matter—may be found fairly close to the surface and yet appear exempt from this source of contamination. It may be that it will ultimately prove more satisfactory to collect carbon from such relatively poor formations

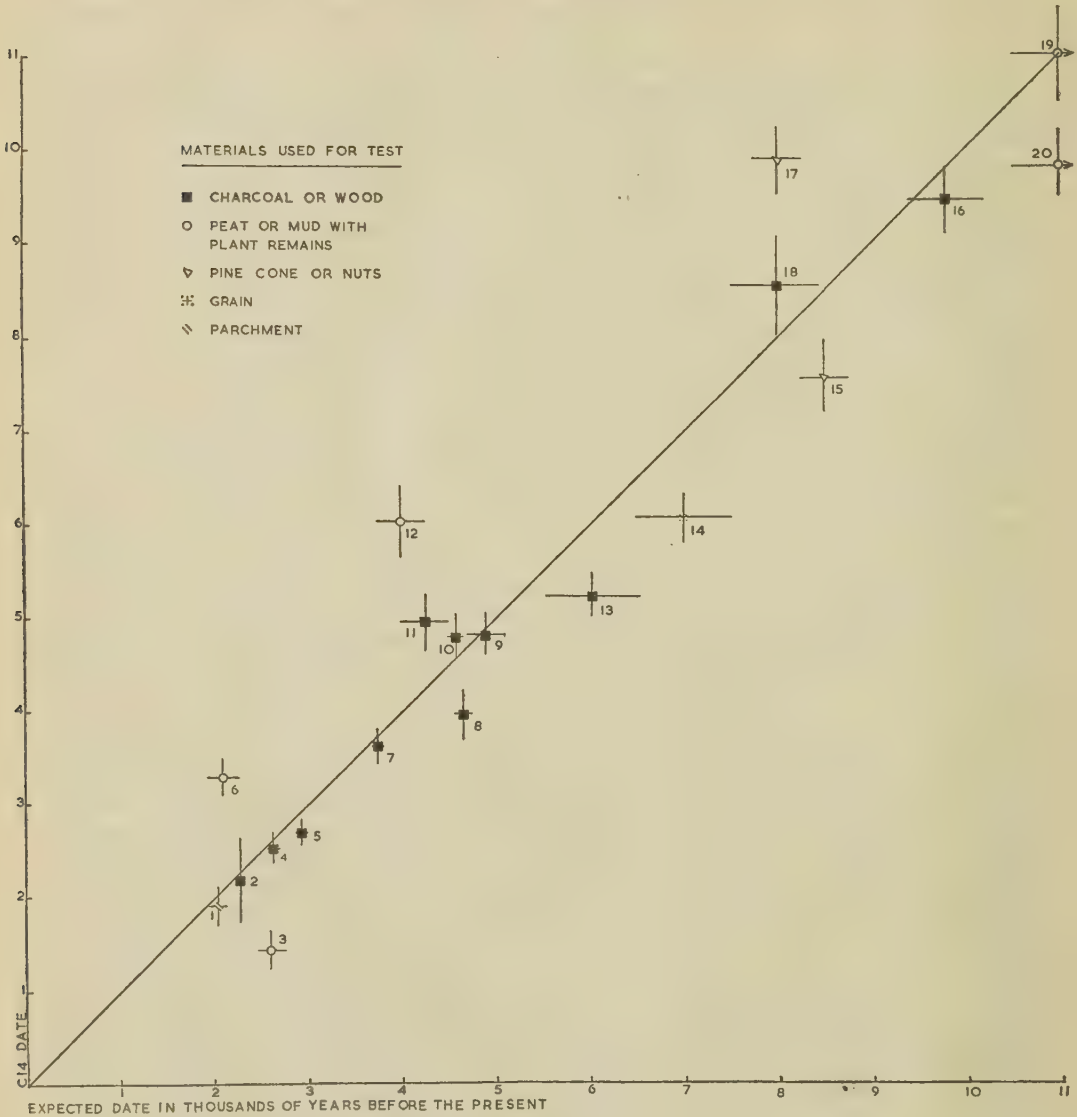


FIG. 1

(using perhaps some chemical means for separating the charcoal from the matrix) rather than from more obviously carboniferous remains such as actual hearths.

On the whole, though theoretical considerations would suggest a somewhat greater

[continued on p. 38]

RADIOCARBON DATING RESULTS FROM THE OLD WORLD

The graph (FIG. 1) shows the relationship between radiocarbon dates and those predicted by historical and geochronological methods.

1. AIN FASHKA, PALESTINE (biblical scrolls).
2. EGYPT (Ptolemaic wooden coffin).
3. MELBACK, GERMANY (peat from *grenz-horizont*).
4. TAYINAT, SYRIA (wooden floor of Syro-Hittite palace).
5. Heart-wood of Sequoia.
6. SHAPWICK, ENGLAND (peat from zone believed to belong to Iron Age to Roman times).
7. EGYPT (fragment of funerary ship of SESOSTRIS III).
8. „ (wooden beam from tomb of ZOSER).
9. „ (beam from roof of tomb of HEMAKA).
10. „ (beam from tomb of SNEFERU).
11. EHENSIDE TARN, CUMBERLAND (charred wood, associated with Neolithic A pottery).
12. SHAPWICK, ENGLAND (peat attributed to the neolithic).
13. EL OMARI, EGYPT (charcoal from Middle Predynastic settlement).
14. FAYOUM, EGYPT (grain from Neolithic A settlement).
15. AAMOSSEN, DENMARK (pine cone from Boreal horizon).
16. STAR CARR, YORKSHIRE (wooden log from pre-Boreal horizon).
17. AAMOSSEN, DENMARK (nuts from late Boreal layer).
18. „ (late Boreal charcoal)
19. HANNOVER, GERMANY (mud from pollen-zone II).
20. HAWKS TOR, CORNWALL (peat from pollen-zone II).

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margin of variability, shells would seem to be preferable from certain points of view. Thus, providing they are in a perfectly fresh condition and unaffected by any kind of chemical disintegration, they are clearly easier to free from recent remains. The actual results so far obtained with shell are as yet insufficient to confirm this, though it will be a matter of some practical importance to archaeologists working in coastal areas to see how far they do so in the future.

Little use of burnt bone has yet been made in even approximately datable contexts, though here again we are warned by the physicists to expect even greater errors than with shell. It is true that a reversed dating has been recorded for successive layers in the newly discovered Belt Cave in Iran¹, but it is perhaps not fair to attach too much importance to this in view of the small size of the samples submitted for examination, and the consequently extremely large margins of error in the observations. Turning from these general considerations to some of the more interesting particular results, the following comments may be offered.

The most interesting date at the lower end of the series, is probably that assigned to the newly discovered biblical manuscripts from Ain Fashkha, in Palestine. It will perhaps be recalled that the extreme dates assigned on palaeographic and literary grounds ranged over some five hundred years, though agreement with G. L. Harding's estimate of 1st or 2nd century B.C. is close, namely 1917 ± 200 before the present.

On the whole the accuracy of the generally accepted chronology in Egypt is impressively supported by the Middle Kingdom and two of the Old Kingdom figures. The reading for Zoser on the other hand— 3979 ± 350 before the present—is so obviously too late that one can reasonably suppose some source of contamination. Of wider import to most European archaeologists will be the two prehistoric dates from Egypt; the first from Miss Caton-Thompson's 'Neolithic A' stage in the Fayoum, and the second from what is described as a 'Middle Predynastic' context at El Omari in Lower Egypt, 4044 ± 205 B.C. and 3205 ± 230 B.C. respectively.

The second classification is sufficiently vague, but assuming that no gross error has been made, it will readily be conceded that El Omari should provide a reading appreciably earlier than the First Dynasty, and unless Miss Caton-Thompson's general succession in the Fayoum is seriously at fault, Neolithic A in that locality should be earlier still. Although both these minimum expectations are fulfilled, yet the absolute dates arrived at will come as a shock to many. The very rough dates² originally proposed for the two stages in question have for so long been accepted as a basis for discussion, that one has tended to forget that in reality they never rested on more than the most tenuous series of deductions. Probably the most operative of these was the notion that the slow pace of cultural change in dynastic Egypt argued a comparable rate of progress in the earlier periods as well. It may however be pointed out that by no means all the characteristics of the prehistoric communities accord at all easily with this conception.

Such for instance are the far more striking regional differences in a variety of industrial practices, such as pottery and flintwork and the far greater apparent effect of traits of foreign origin. Again is it not perfectly possible that, once the initial discovery of food production had been made, industrial and social development may have proceeded at first at a relatively rapid rate? It is obvious that two entirely new factors in the situation were introduced at the beginning of dynastic times both in Egypt and Mesopotamia,

¹ See *Cave Explorations in Iran*, 1949, by C. S. Coon, the University Museum of Pennsylvania, Philadelphia.

² Hitherto sometime in the 6th millennium B.C. has been regarded as the most probable date for Neolithic A, while a late 5th or early 4th has been envisaged for the Middle Predynastic.

RADIOCARBON DATING RESULTS FROM THE OLD WORLD

namely widespread political uniformity and comparative saturation of the economic resources of the country. It needs no great imagination to see in these the main cause of the subsequent slowing up of the processes of cultural change.

However this may be the new list of dates certainly makes it seem increasingly likely that the only real hope of enlightenment on equally important topics connected with the 'Neolithic industrial revolution', such as the relative age of the earliest traces in different areas, and the direction of diffusion of basic practices, lies with an adequate series of carbon analyses for each stage and region. The most interesting of the dates already available outside Egypt, are probably those provided by Jarmo and the Belt Cave. At Jarmo, a stratified occupation mound or *tell* in the Kurdish hills north-east of Mesopotamia, are traces of a village settlement which is believed on typological grounds to represent the earliest stage of such communities so far identified in Western Asia. The carbon date, derived from land snails, is given as 6707 ± 320 before the present. Using double the margin of error this would indicate a period between the middle of the 6th and end of the 5th millennia B.C.

It has however been pointed out, that it is not in *tells* that we may expect to find the traces of the beginning of the Neolithic, since it is clear that the architecture of villages must have been invented later than the technique of food-production which made permanent settlements possible. It is accordingly in repeatedly occupied sites of the type used by hunter-gatherers, such as caves or spring-side camping areas that we may be able to identify the true horizon of the initial discoveries. One such seems to be provided by the newly investigated Belt Cave on the southern shores of the Caspian, referred to above. Unfortunately the assays—dating the earliest signs of the domestication of sheep and goat—are based on undesirably small samples, with a correspondingly large margin of error, namely 8085 ± 1400 before the present. These remains are stratified in a continuous sequence above layers showing unmistakable evidences of a purely hunter-gatherer culture of 'mesolithic' type.

No dates are yet available for the equivalent stage in Palestine or Syria, or the first village settlements of Mesopotamia, but remains believed to represent the first settled culture on the Anatolian Plateau, namely the 'Chalcolithic' layer at Alishar, are dated to 4519 ± 250 before the present. Though perhaps not inconceivable this is certainly considerably later than expected, and will require further confirmation from other sites before it can be accepted with confidence.

Second only in interest to the origin of the Neolithic is the problem of the dates of the Upper Palaeolithic in Western Europe. Investigation of this has yet scarcely begun, and two major difficulties have to be faced. First, as already mentioned in the previous article, the maximum period which can be measured with anything approaching confidence as yet is in the order of 20,000 before the present, a date which cannot be far removed from the figures which it is desired to establish. Secondly it is clear that such minute reactions as those yielded by samples of this order of antiquity will be disproportionately affected by a degree of contamination which, at a later stage, would be quite insignificant.

The single figure for the French Upper Palaeolithic is that of 15516 ± 900 before the present, for remains attributable to the latest Gravettian or earliest Magdalenian stages from the famous decorated cave of Lascaux. Opinion is already sharply divided on the significance of this date, which by many is regarded as far too late.¹

¹ As we go to press the following dates before the present are released for the Magdalenian III horizon at La Garenne (Neuvy St. Sepulchre, Indre), 11109 ± 480 , 12986 ± 560 (both from carbonised bone), and 15847 ± 1200 (ashy matter mixed with sand). (See *Mem. Soc. American Archaeology* No. 8, July 1951; the first date and locality omitted in error).

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A possible approach to this question is afforded by the results offered for the various post-glacial climatic stages. Where these are based on peats and muds they seem to be highly variable to say the least— 6555 ± 280 for pollen zones II to III in England for instance, whose expected date by an extension of varve chronology would have been approximately 10300. On the other hand a wood sample from Dr Graham Clark's well-known site at Star Carr in Yorkshire, attributed to pollen zone IV (the Pre-Boreal) with an estimated age of about 9800 before the present, yields a carbon date of 9488 ± 350 . Carbon dates for the earlier Allerød stage, given as 11044 ± 500 (Hannover, Germany), 11310 ± 720 (Knocknacron, Ireland), 9861 ± 500 (Hawks Tor, Cornwall), and 11442 ± 640 for the correlated stage (Two Creeks) in America, seem to agree substantially with the varve date of approximately 11000 ± 500 . The results for the Boreal phase immediately following that of Star Carr are given as 7583 ± 380 , 9929 ± 350 , 8631 ± 540 , 9425 ± 540 (all from Denmark) and compare with 7600–8800 estimated from varves.

Earlier Late Glacial figures are few, though it may be recalled that the *varve* dating proposed for the Hamburgian archaeological stage of North Germany runs approximately from 14500–15500 before the present. On archaeological grounds the Hamburgian has been claimed by A. Rust to be closely allied to the later Magdalenian of France, though this is contested by some French authors. If Rust is right, however, it must be admitted that the whole scheme from Lascaux to the later Post-Glacial stages is not without considerable consistency.

One other palaeolithic dating given in the list has been the subject of some comment, namely that of Fontmaure, France, where the date of the Mousterian is given as 973 ± 230 . With regard to this site the writer can only state that when he examined the collection in 1949³, he formed the strong opinion that it consisted of a mixture of Mousterian with specimens of much later character, in all probability mesolithic. The site is not 'a cave' as described in the catalogue of carbon readings, but an open site in which the finds occur approximately 60 cm. from the surface, and it may be added that the identification of the charcoal in question showed only three species—two of oak, and one of chestnut⁴.

The Ponders End deposits in the Lower Thames, correlated with the Late Levalloisian or Mousterian by King and Oakley⁵, is the only serious instance of dated deposits equated with this culture stage, and provided a date of 'over 20,000'.

Of later prehistoric readings in Western Europe the following may be mentioned. A piece of charred wood from Ehenside Tarn suggests a date of 4964 ± 300 (3314–2714 B.C.) for Neolithic 'A', which seems not wholly inconceivable. Peat attributable to an Iron Age to Roman period from Shapwick, Somerset, and providing a date of 3310 ± 200 (1560–1160 B.C.), however, needs no comment. A sample of beeswax said to have been found with Late Bronze Age specimens, which dated to 819 ± 160 before the present, is also not encouraging for the use of this material.

In conclusion it may be said of the Old World samples so far available, that although some correspond to accepted notions quite nicely, and others are obviously quite wide of the mark, there remains a considerable intermediate category which if correct, would require, to put it mildly, a considerable readjustment of current views. The upshot of the contest will be eagerly awaited by archaeologists in many different fields; but at least it may be said that results of wide significance should certainly follow as soon as well attested readings are sufficiently numerous to allow of adequate statistical treatment.

³ Before there had been any suggestion of taking a carbon sample.

⁴ Pradel, *Bull. Soc. Prehist. Française*, 1947, pp. 28–30 and 1945, pp. 84–93.

⁵ King and Oakley, *Proc. Prehist. Soc.*, 1936, pp. 66–7.

Notes and News

RINGHAM LOW: THE REDISCOVERY OF A DERBYSHIRE CHAMBERED TOMB¹

Ringham Low (Monyash) was recorded by Bateman in the mid-19th century as a barrow of considerable size and importance² (FIG. 1). He shows plans of the barrow and of four of the chambers which it contained. At that time continuous stone-robbing

RINGHAM LOW

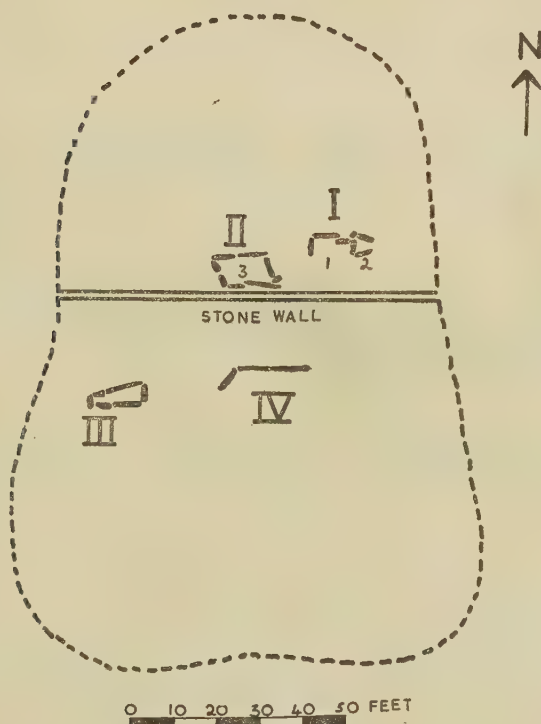


FIG. 1

was taking place, so that, when Ward wrote forty years later³, the chambers had all been destroyed. Since then, the location of the site itself has been forgotten and it could not

¹ This note is published in anticipation of a general paper on the Peak collective tombs, which has been delayed by the authors' absence abroad. It is hoped that other field-workers will now have an opportunity to give the site the thorough examination which it deserves.

² T. Bateman, *Vestiges*, p. 103, and *Ten Years Diggings*, p. 94, et seq.

³ *V.C.H. Derbyshire*, Vol. 1, J. Ward, 'Early Man', pp. 165-8.

be discovered by Phillips⁴. Recently, local enquiries revealed, however, that a few fields lying to the east of Ricklow Dale still bear the name Ringham Low. Within these, about 650 yards to the east of Ricklow Dale and 450 yards to the north of Lathkill-dale, the O.S. shows a small plantation with tumuli (MR 170/655), which must clearly be Bateman's site. The barrow lies partly beneath this plantation, partly to the north. To the north of the plantation, the barrow has almost completely disappeared, and no signs of chambers remain. Within the walled plantation, the stony ground is covered with undergrowth and any structures are now indefinite. Confusion has probably arisen

RINGHAM LOW

CHAMBER I

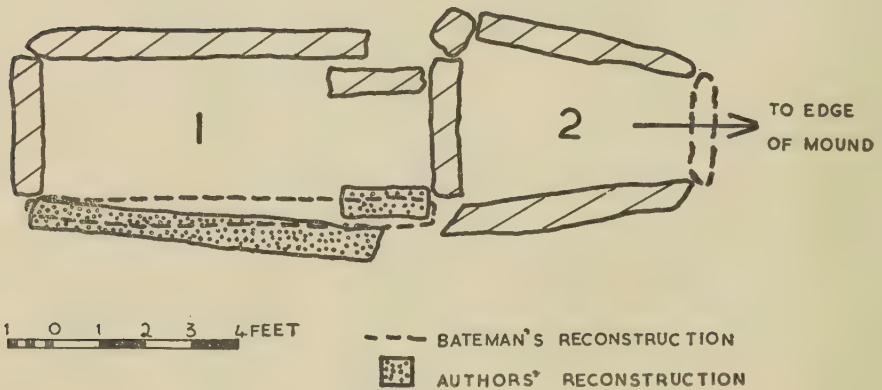


FIG. 2

as Bateman records Ricklow Dale as lying to the east of the barrow; in fact the barrow lies to the east of the dale.

Daniel⁵ dismisses Ringham Low as a group of large cists, but it seems more likely that one at least of the structures is a chambered tomb of the usual Peak type⁶. This chamber (FIG. 2) consists of two cells, containing six skeletons; it had already been disturbed when Bateman planned it, and the south wall of cell 1 was missing. Bateman

⁴ *Ordnance Map of the Trent Basin* showing distribution of long barrows, megaliths and habitation sites, pp. 10, 17 and 22. It should be noted that the map requires modification as the site is to the east of Monyash.

⁵ G. E. Daniel, *The Prehistoric Chamber Tombs of England and Wales*, p. 183.

⁶ The type, single-celled, wedge-shaped and entered between two pillar-stones from a passage was discussed by Ward, writing on Five Wells (J. Ward, *Reliq. and Ill. Arch.*, N.S. VII, p. 229-42). Daniel, relying exclusively on plans, has obscured several important features. The whole question will be discussed at length in the paper already referred to. It should be added that the Bride-stones has no *morphological* claim to a place in the Peak group.

attempts to reconstruct the chamber but he gives no evidence for his plan. He makes cell 1 rectangular and cell 2 wedge-shaped; it would seem more reasonable to reconstruct the south wall of cell 1 on the analogy of the north wall, where one long slab overlaps a much shorter one. This would give a slightly tapering chamber entered through two pillar-stones closely comparable with the chambers at Minninglow and at Five Wells⁷. Cell 2, which is towards the border of the mound, may be a subsidiary chamber, or, as the authors hope to show in a general discussion on these tombs, the remains of a passage. Bateman does not give the heights of the individual stones, but he does give the depth of each cell; in the case of cell 1, 3 feet 6 inches, and of cell 2, only 2 feet. These measurements, which presumably show the average height of the side-walls in each case, would make both cells very low, but they may have been built up with dry-stone walling.

Two leaf-shaped arrowheads were found in cell 1 in association with four of the skeletons, and three were also found in the interstices between the paving stones of Chamber II. This chamber contained twelve bodies and is clearly a second collective tomb, but it is not possible to make anything of Bateman's plan and description. There were no other finds of interest in the barrow, but similar arrowheads were also found by Ward at Harborough⁸, and are common in megalithic tombs. In short, Chamber 1 at Ringham Low provides another example of the type of collective tomb characteristic of the Peak, and it may be that careful excavation would reveal the original form of the barrow and its chambers.

LESLIE AND ELIZABETH ALCOCK.

ROMAN COINS FROM ICELAND

The discovery of Antoniniani of the emperors Aurelianus, Probus, and Diocletian on Iceland which has been discussed in this Periodical two years ago¹ and which appears to be connected with a find which is beyond suspicion came rather surprising because human settlements on this North Atlantic island are attested by our written sources for as late a time only as five or more centuries after Diocletian. My predecessors in discussing the Iceland coins have tentatively connected this new evidence with Roman Britain, either by attributing the first certain visit of Europeans to Iceland, which appears to be indicated by the three Antoniniani, to Romano-British seafaring merchants or even to a Romano-British warship of the separatist Roman emperor of Britain, Carausius.

Unfortunately these scholars did not look up the mints by which the three coins were issued originally and which can be established with certainty for Antoniniani as a rule. From the most up to date survey of Roman Imperial Coinage which we possess for the later 3rd century A.D. at the moment—that of Mattingly and Sydenham²—the following information about the well known and fairly common types of the Iceland coins has to be added to the earlier detailed descriptions of the specimens by Shetelig and Eldjárn:—

⁷ Most easily accessible in Daniel, *op. cit.*, p. 85, fig. 24.

⁸ J. Ward, *D.A.ŷ.*, 1890, Vol. XII, p. 118, et seq.

¹ Cp. with earlier bibliography, H. Shetelig, 'Roman Coins Found in Iceland', *ANTIQUITY* XXIII (1949), 161 f.; K. Eldjárn, 'Fund of Romerska Monter Pa Island', *Nordisk Numismatisk Årsskrift* (1949), 1 f. I have to thank my pupils, K. W. Stubington for having translated Eldjárn's rather instructive article into English, and especially F. H. Armstrong, B.A., Assistant in the Royal Ontario Museum of Archaeology who was most helpful in checking Eldjárn's coin descriptions with the relevant specialist numismatic literature.

² Cp. for the numismatic problems discussed in this article H. Mattingly-E. A. Sydenham, 'The Roman Imperial Coinage', vol. v, 1 (1927); v, 2 (1933); K. Pink, 'Probus', *Numismatische Zeitschrift*, 73 (1949), 13-74.

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1. Antoninianus of Aurelianus. Mint of Cyzicus. Cp. Matt.-Syd., *op. cit.* v, 1, p. 306, no. 361.
2. Antoninianus of Probus. Mint of Rome. Corr. on Rev. RA to R Δ . Cp. Matt.-Syd., *op. cit.* v, 2, p. 37, no. 187.
3. Antoninianus of Diocletian. Mint of Rome. Cp. Matt.-Syd., *op. cit.* v, 2, p. 236, no. 161.

It appears from the foregoing that the two younger Antoniniani were minted in the city of Rome itself with certainty. On the other hand no. 1 seems to have been issued at Cyzicus on the Asiatic side of the Propontis, a mint which was of importance both for Asia Minor and the Balkan peninsula. A careful inspection of the originals in Iceland by an experienced numismatist may still lead to slight corrections in their attribution to catalogue varieties. Nevertheless already now it is obvious that none of the three Antoniniani was minted in Britain, Gaul, or Spain. Under these circumstances it is most unlikely that they were, as has been suggested in earlier articles, the property of a Romano-British or Western provincial merchant who had acquired them by buying and selling some merchandise in the western part of the Roman Empire with its numerous imperial mints.

Similarly the naval ratings of the fleet of the rebellious emperor Carausius who appear as *dei ex machina* in Eldjárn's theories about the last owners of the coins were certainly not paid with Antoniniani from mints which were controlled by the inimical emperors Diocletian and Maximianus Herculeus. Instead it looks in my opinion as if the owner or the owners of our three coins had lived in Asia Minor or near the Danubian frontier of the Roman Empire originally, but took residence in Rome during the years between the later reign of Aurelianus and the earlier years of Probus, and left the eternal city and the territory of the Imperium Romanum from unknown reasons for adventures on the Atlantic Ocean under Diocletian.

Most attractive is, under these circumstances, to presume that the owner or owners of the three coins were first members of the Roman field armies on the Danube or in the Orient and later of the garrison of Rome, all three military establishments being strongly recruited from free Germany near A.D. 300, the approximate date of our find³. Perhaps the three coins were left on Iceland by members of the crew of a Scandinavian or Saxon ship who had served in the Roman army in earlier time, but were now raiders, pirates and adventurers on the Atlantic with the result that their ship was driven by a storm close to the still undiscovered American continent. To bury votive gifts in times of distress, as was presumably done with the three Iceland Antoniniani, is a well known Germanic custom which leads back to the Neolithicum of Northern Europe⁴. The new coin find from Iceland is therefore best interpreted, in my opinion, as evidence for early Germanic visitors to the remote island.

The much discussed Thule problem has to be reconsidered in the light of the new evidence also. The island of Thule of which the Greek merchant adventurer Pytheas of Massilia was told in Britain near 320 B.C. has been considered to have been Iceland by many serious scholars for a long time⁵. The main obstacle for such an interpretation

³ Cp. with earlier bibliography A. Schenk von Stauffenberg, *Das Imperium und die Völkerwanderung* (1947), 7 f., 212 f.

⁴ Cp. K. Helm, *Altgermanische Religionsgeschichte*, I (1913), 243 f.

⁵ Cp. with full bibliography M. Cary-E. Warmington, *The Ancient Explorers* (1929), 33 f., 36 f.; J. O. Thomson, *History of Ancient Geography* (1948), Index s.v. Pytheas, Thule; Pauly-Wissowa, 'Realenzyklopädie der klassischen Altertumswissenschaft', art. Thule; *Oxford Classical Dictionary*, art. Pytheas, Thule.

NOTES AND NEWS

of Pytheas's report was that honey from bees was supposed to have been obtainable on this Thule, Iceland being too far to the north for the varieties of *apis mellifica* to survive. But recently the plausible suggestion has been made that Pytheas's honey was collected, not from domesticated, but from the hardier wild bees which indeed belong to the fauna of Iceland⁶.

Furthermore if Iceland could be reached by ships from Europe in A.D. 300 or so as is now practically certain, then there is no reason to doubt that the island was approached occasionally by the already similarly well-built vessels of the Hellenistic Age also. The discovery of the three Antoniniani makes it therefore as certain as the general state of our scanty and all too derivative written sources permits that Pytheas of Massilia and the rather sceptical Graeco-Roman readers of his published reports during the Hellenistic and Roman centuries actually had some information about Iceland at their disposal. Pytheas, this extraordinary seafarer of Marseilles, might have discovered America long before the Vikings and Columbus if he had continued his journey in a westerly direction.

F. M. HEICHELHEIM,
Toronto University.

⁶ M. Cary in *Classical Review*, 63 (1949), 112.

Important New Books and Articles

The inclusion of a book in this list does not preclude its subsequent review

- DREVNAYA ISTORIYA YUZHNOY SIBIRI, by S. V. KISELEV. No. 9 of the Series which started as *Matériaux et Recherches d'Archéologie de L'U.R.S.S.* but has now dropped its French title (!) and bears date Moscow, Leningrad, 1949, 4to, 364 pages, 62 plates, many maps. [A fine coordination of the history and archaeology of Southern Siberia from neolithic to Mongol times. French resumé by R. Ghirshman in Salmony's *Artibus Asiae* xiv, 1951, 169-89].
- BANDKERAMISCHE STUDIEN, by AUGUST STIEREN and EDWARD SANGMEISTER : 33 Bericht der R.-G. Kommission. [The authors adduce evidence to justify the thesis that the earliest cultivators in Central Europe (the Danubians) lived not in irregular pit-dwellings but in very long houses ; and they draw highly plausible inferences about the organization, population and duration of these settlements].
- ENGLISH SOCIETY IN THE EARLY MIDDLE AGES, by DORIS MARY STENTON. *Pelican*, 2s 6d. [A description, drawn largely from contemporary sources, of England in the 2½ centuries after the Norman Conquest, its government, the life of rich and poor, its economy and its Church affairs. A book by Lady Stenton does not need our commendation ; it is a wonderful book for so small a price—the harvest of a lifetime's work, cast in a popular mould].
- KHARGA OASIS IN PREHISTORY, by G. CATON-THOMPSON, with a Physiographic Introduction by E. W. GARDNER. Univ. of London ; Constable & Co., 12 Orange St., London, W.C. 2, 1952.
- PREHISTORIC EUROPE : the Economic Basis, by J. G. D. CLARK. 350 pages, 16 plates, 180 text illus. Methuen & Co., 1952.
- LES VOIES ROMAINES DE L'AFRIQUE DU NORD, by PIERRE SALAMA. Gouvernement General de l'Algerie : Service des Antiquités. Algiers, 1951 : 143 pages, 12 half-tone plates, map (in colour) 1 : 500,000. [We have not seen this book because the publishers, who have probably not yet discovered ANTIQUITY, have not sent us a copy ; but we are told that it is an 'exhaustive survey with gazetteer of Roman sites, and very useful, as the corresponding sheets of the Tabula Romana are not yet published'].

Reviews

MAP OF MONASTIC BRITAIN: Scale 1:625,000: *published by the Ordnance Survey, 1950, two sheets. 7s 6d per sheet.*

This latest addition to the series of National Period Maps published by the Ordnance Survey has been compiled by Mr R. Neville Hadcock. The two sheets shew the geographical distribution and the character of the monastic houses of Great Britain between the Norman Conquest and the Dissolution of the Monasteries. Each sheet is prefaced by a short introduction, which lists the authorities consulted, and a combined index to the two sheets.

For the initial date 1066, has been chosen so that the map gives a full picture of the new and reformed orders, together with the Benedictine houses which had been founded at an earlier date. In this sphere Mr Hadcock's work is thorough and exhaustive. The Director General in his foreword appeals for corrections by students of local history; some will surely be forthcoming, but they are likely to be few and of little importance. A random examination in several districts has shewn that the lists and classifications need little revision. Two minor corrections may be noted. There is some evidence that Souleseat, correctly shewn as a Premonstratensian house, was first founded for the Cistercians. In Devon Harberton and Bothemescombe, indexed as alternatives, are separate sites. The latter lies in the parish of Uffculme (now Bodmiscombe) and seems to have been the principal holding of the Hospitallers in Devon.

In Scotland and Wales the date 1066 is less satisfactory. Older customs survived in these areas to a much later date and the attempt to shew these survivals on the map is neither consistent nor comprehensive. It would have been simpler to adopt the rigid medieval theory, which did not regard the *clas* or its equivalent as a monastery in the full sense of the word. The result would have been the deletion of many signs that confuse the main issue and do not really add to our appreciation. A few examples must suffice. In the north the life and writings of St. Ailred shew that communities of priests or monks survived into the 12th century at Kirkcudbright and Whithorn; neither is indicated on the map alongside the later reformed Orders. In Wales, Holyhead is shewn as a collegiate church, while Clynnogfawr in addition to the same symbol has a second for the extinct *clas*; yet these establishments have a similar history, the primitive *clas* becoming a collegiate church in the course of the Middle Ages. The choice of sites to be marked with the symbol of a *clas* is also open to question. Churches, like Corwen and Llansannan, which were portionary in 1291 do not appear on the map, though they have a better claim than an ancient church like Llandrillo, which had certainly ceased to be a *clas* by the early 13th century. There are similar anomalies in Cornwall, where Bodmin, though marked as Augustinian house does not receive the symbol of an extinct *clas*.

A kindred point arises in the text about the Scottish sees, a paragraph which is too condensed to cover a very complex subject. The omission of Aberdeen is clearly an oversight, but Galloway was not revived till c. 1125. There is evidence, admittedly of uncertain value, that both Orkney and Aberdeen have a succession reaching back before 1066. Furthermore the classification together of the 'additional sees' tends to destroy the contrast between new creations such as Caithness and Moray, which cover unified areas, and sees like Brechin and Dunkeld, which are composed of scattered districts and can be most readily explained as the survival of Celtic bishoprics of the older, jurisdictional, non-territorial type.

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These criticisms refer to a minor and non-essential feature of the map. They do not detract from its very real value as a picture of an essential part of the ecclesiastical life of the Middle Ages. The symbols are clear and well chosen and the general appearance and execution of the sheets fully conforms to the very high standard, which Ordnance Survey has set itself in this series. C.A.R.R.

MISCELLANEA MUSICOLOGICA (I-II), 1948. 25 pages, 2 plates, 22 figures.

MISCELLANEA MUSICOLOGICA (III-VI), 1949. 31 pages, 7 plates, 12 figures.

CYMBALES ET CROTALES DANS L'ÉGYPTE ANCIENNE, 1949. 95 pages, 10 plates, 51 figures.

CATALOGUE GENERAL DES ANTIQUITÉS ÉGYPTIENNES DU MUSÉE DU CAIRE, Nos. 69201-69852, INSTRUMENTS DE MUSIQUE, 1949. iv-216 pp. 116 plates, 74 figures.

All by DR HANS HICKMANN: printed in Cairo by Imprimerie de l'Institut Français d'Archéologie Oriental for the Services des Antiquités de l'Égypte.

LA CLIQUETTE, UN INSTRUMENT DE PERCUSSION ÉGYPTIEN DE L'EPOQUE COPTE. By DR HANS HICKMANN, Cairo, Imprimerie de l'Institut Français d'Archéologie Oriental 1950 for Société d'Archéologie Copte. 12 pp, 8 figs. No prices given.

Of the *Miscellanea*, nos. I, II, V, VI, describe the structure and methods of tuning of Ancient Egyptian harps, lutes and lyres, incidentally making it clear that tuning by movable pegs is a comparatively modern innovation. Pegs were used merely as anchors for the strings which were tuned by being wound round the necks of the instruments, or unwound. The harp is the most characteristic instrument of Ancient Egypt, but Dr Hickmann shews that it is the lute in its various forms which is the ancestor of most modern stringed instruments. No. IV of the *Miscellanea* argues that certain perforated shells found in prehistoric burials were used as whistles.

No. III of the *Miscellanea* is an extremely interesting description of the way in which a contemporary Coptic church singer accompanies his singing with movements of his hands (chironomy) to remind choirs or pupils of the rhythm and shape of the phrases they are singing or learning. The singing is alleged to be in Ancient Egyptian and Early Christian style, and the author deduces that persons depicted by the Ancient Egyptians as appearing to conduct other musicians are in fact chironomists. The various forms of musical notation derive from chironomy. This surely is a performance which should be recorded on a sound film before the technique disappears.

The Monograph on Cymbals and Crotales (small bells) is devoted mostly to Cymbals, and describes all shapes and sizes. These instruments date from the New Kingdom, Greco-Roman and Coptic periods only, and were imported from Asia, having originated from ritual cups and censers.

The Catalogue appears to be a very complete one, and includes sound-making objects and bell moulds. The instruments are classified in groups, and appendices giving cross references which make it simple to trace any item. Every type of instrument is fully illustrated.

Many examples of the Cliquette or Clappers survive, but mostly in pieces so that they have not normally been recognized as musical instruments. They are in fact similar to handled Castanets. The author shews them to have been used in Coptic church services, and to have regulated Monastic life as the precursors of bells during the 3rd to 6th centuries.

P. J. M. DAVIES.

MONMOUTHSHIRE HOUSES : PART I, MEDIEVAL HOUSES. By SIR CYRIL FOX and LORD RAGLAN. *The National Museum of Wales, Cardiff*, 1951. pp. 114, 20 plates, 54 text figures and 4 sketch maps. Price, 17s 6d.

The scientific study of peasant building in the British Isles has long been the Cinderella of our folk culture. Addy in 1910, *The Evolution of the English House*, and Innocent in 1916, *The Development of English Building Construction*, made excellent pioneer studies and since then there has been a steadily growing literature, much of it in the journals of local societies. In the first part of their work on Monmouthshire houses, which covers the period from 1415 to 1560, Fox and Raglan have produced a thorough survey of the small houses of the rural hinterland of that county.

Cruck-framed buildings provide one of the most interesting of our early types and both Addy and Innocent dealt fairly fully with this structure. Innocent defined its southern limit as a line drawn from the Wash to the Bristol Channel but since then the assiduous collecting of N. Teulon Porter (unpublished material in the possession of the reviewer) has extended the boundary far to the south and west of this line. The earliest dated English example is the Enstone tithe barn, which was built in 1382, but there is a record of 'two bent beams called "crockes"' at Harlech in 1278 and Henblas, Llandderfel, Merioneth, is considered to be early 14th century. On the Continent, Klein has described a cruck building from Westphalia dating from A.D. 350 and the work of Trefois, van Giffen and others has established cruck distribution extending in a band some 45 miles wide from West Flanders to Denmark.

The major part of the book under review is devoted to a description of thirty-seven cruck-framed buildings in Monmouthshire. The earliest example, that of Pistyll, Tegare, is placed between 1420 and 1450. It is a clumsy truss with wide feet, a single low collar beam and no tie beam. Subsequent development resulted in a characteristic paring of the cruck foot and either the use of the tie beam to support the wall-plates or the more interesting innovation of slotting heavy oak spurs, notched and shouldered into the crucks, to carry the wall-plates. The peculiar reduction of the cruck feet is ascribed to the intrusion of the heavy timber-frame house-building tradition which replaced the earlier light wattle and daub. Ultimately the cruck truss was reduced to an 'upper cruck' with a much more angular bend and a shorter upright portion which was tenoned at its foot into the ceiling beam. The authors have established this development thoroughly and scientifically but their work is not confined to the cruck framework alone; it includes an equally detailed treatment of the house-plan and all the attendant features of wall structure, doorways, windows, fireplaces and staircases.

The second part of the book deals more briefly with small medieval stone houses and in Monmouthshire these have a central house-body, separated by a passage at one end from the kitchen and with a solar at the opposite end. The peculiarity of these houses lies in the siting of the house-body fireplace in the centre of the passage wall, a feature common to the 15th century Pennine houses of the same plan. Fox and Raglan suggest that this type is more common, if not peculiar to, the highland zone and they emphasize the difference between this arrangement and the normal 'screens passage' where one end of the hall is partitioned off by wooden screens and the fireplace is sited in the side wall. It is unfortunate that they have used the term 'screens passage' for the passages in the Monmouthshire houses as these were probably not the result of simply screening off the entrance end of the hall. The association of a pair of opposite doors with the house-body fireplace is of some significance and indicates that this type of passage probably originated as a means of controlling the fire as I have already suggested for the Pennine houses (*Country Life*, Dec. 3, 1948). The use of a pair of opposite doors

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for this purpose has been noted by Campbell in Ireland (*Folk-liv*, 1937), by Erixon in several parts of Western Europe (*Folk-liv*, 1937) and by Peate in the Welsh long-house (ANTIQUITY, 1936). Fox and Raglan themselves suggest the possibility of such passages having resulted from the assimilation of the hall-house-with-spere-truss and the long-house plans, although definite proof for this is still lacking.

It is a book full of sound scientific reasoning and one which makes the reader eagerly await the publication of the next two parts. If there is any criticism to be made it concerns the plates. Many of the photographs, such as IIC, IID, va and VIIC, do not appear to have been taken with any specific object in view and do not afford even a good general impression of the subject. Sir Cyril Fox is a very good draughtsman indeed and many of the photographs of details, such as the door frame of Upper Trerhiw (Plate XIIB) and the solar window of Blaengavenny (Plate XIVB) would have been much clearer and far more useful if depicted as line drawings. But these are very minor criticisms of a fine book and a valuable contribution to our study of British folk architecture. Certainly no such criticism can be levied against Sir Cyril Fox's line drawings which are a model of clearly expressed architectural information. The National Museum of Wales has done a great service to students of folk culture by the publication of such a regional survey which will serve as an admirable guide for similar studies in other parts of the country.

JAMES WALTON.

METALLURGY IN ANTIQUITY. By R. J. FORBES. 489 pages, 98 figures including maps and diagrams. Leiden (E. J. Brill), 1950. Price Gld. 19 (= about £1 16s 5d).

A reliable book on metallurgy from the earliest times has long been a desideratum, and Mr R. J. Forbes has made a gallant effort to supply one. For several reasons he cannot be said to have filled the gap, the chief being that, as he points out in his last chapter, a great deal of research work still remains to be done. Modern methods can tell us a great deal about the origin and the method of manufacture of metal objects, and they are now usually applied in every important excavation; but 'what we need most is a thorough investigation of all the metal remains now buried in our museum store-rooms (p. 477)'. It is true that even microchemical and spectrographic methods necessarily destroy some of the original material, but the amounts are minute. Metallographic examination by grinding down till metal unaltered by surface changes due to conditions of burial, etc., leaves a small area looking much as it did when the object was in use; but surely that is not a serious objection, even if a 'beautiful patina' is thereby destroyed. One great virtue in Forbes's book is the indication of what might be accomplished by collaboration and organisation. What an immense amount of knowledge might be gained if the equipment and the staff of Dr Plenderleith's laboratory could be multiplied by ten!

For practical purposes, however, the effect of a valuable book depends, not only on the amount of information it contains, but also on the number of people who buy it and read it. Unfortunately 'readable' is not a word that can be applied to 'Metallurgy in Antiquity'. One would not judge harshly the English syntax, grammar or punctuation in a book produced in Leiden, even though the publisher is E. J. Brill; but much more care and time should have been devoted to proof-reading: on p. 91 we are told that 'he (Hephaistos=Ptah) was said . . . to have reigned Egypt and some authors like Johannes Malalas and Johannes Antiochenus even contend that he learnt the Egyptians to forge iron weapons'. That sentence illustrates several points: the marvellous bibliographical learning of the author and a serious lack present throughout his book. We willingly accept quotations from unusual authors, but they have little

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value for us unless the approximate date is given. I must confess that I had never before heard of either of the above authors ; but from the *Encyclopaedia Britannica* I learn that I can see a text of the 6th century Byzantine John Malalas in Migne, *Pat. Graec.* xcvi, of John Antiochenus I learn—nothing. Comparatively well-known classical authors are mentioned in an un-English manner—we do not speak of Cassiodor or Diodor.

For most of those interested in the development of metallurgy it would be a real help if approximate dates were given for most of the authors quoted. A warning is rightly inserted in the introductory note that early chronology is often still uncertain, e.g. that the reign of Hammurabi has recently been brought a century and a half later than used to be accepted, but many readers will often be puzzled even after consulting the bibliographies to each chapter : Agricola's *Bermannus* is rightly given as of 1546, but the much more important and better known *De Re Metallica* appears as Berlin, 1928 ; the original date, 1556, is essential to the relevance, and a book in English should give the Hoover translation (London, 1912) rather than a German edition. *Biringuccio* is once (p. 373) entered as ' Venice, 1540 ', but usually only under a German edition as 1925 (no doubt the American translation is too recent for inclusion). Theophilus *De Diversis Artibus* is often quoted, but is listed only under the name of Theobald, a German editor with date 1933 ; he should appear as XI cent. A.D. (English translation, 1847).

On many subjects antiquaries nowadays are apt to neglect the evidence of the Hebrew literature, perhaps because ' the Bible is Theology '. Forbes, on the contrary quotes every available allusion from the Old Testament, but again with no dates. The well-known description of metal-mining in ' Job ' is, of course, made use of ; but the addition of such words as ' (now regarded as of about 450 B.C.) ' would be an improvement. Some students of early metallurgy might have nothing to refer to but Ussher's dates in the A.V. and think that the passage applied to a date before 1000 B.C.

Despite these criticisms the detailed bibliographies, rightly used, are a most valuable feature of the book ; they include many important papers, in several languages, dealing with every branch of the subject.

The first five chapters are general surveys of the subjects involved, metallurgy, mining tools and methods, including an excellent discussion on fuels (pp. 105-112) and a remarkable account in chap. IV of ' the evolution of the Smith, his social and sacred status ', literally from China to Peru. The greater part of the book is divided into chapters for each metal or group of associated metals. Gold of course comes first, followed by Silver and Lead ; Tin, Antimony and Arsenic ; Zinc and Brass ; Copper ; and finally Iron. For Gold and for Copper the words ' In the Ancient Near East ' form part of the chapter title. In fact throughout the book the great emphasis is on the Near East, justifiable perhaps since the great discoveries in metallurgy in the strict sense can nearly all be proved to have arisen there, but somewhat disappointing to the English reader. One might well expect more attention and fuller references to the important Roman works in Britain and to the inferences that can be safely drawn concerning those of the Bronze and Iron Ages. For each chapter there is a useful map showing known sources of ores and, by arrows, the probable directions for imports and exports ; but the western margins barely touch the mainland of Greece and the northern fail to cover the Black Sea : for tin, an arrow indicates in the Levant possible imports from ' Spain, Bretagne, Cornwall '. Could we not have been given a map showing these mining areas and the proved tin routes ?

On Zinc, Forbes quotes Diergart that the passage in Strabo on ' pseudargyron, which added to copper makes oreichalkos ' is ' technical nonsense ', and denies any

reference to metallic zinc. Apparently he has missed the important discussion on the question in Walter Leaf's *Strabo on the Troad*. Leaf set a splendid example to all commentators on classical literature: he consulted technical experts on the processes employed and Turkish mineralogists on the availability of ores. The only 'nonsense' is a single word—the preliminary calcining of the ore does not produce *iron*, but zinc oxide. But this substance may have been colloquially called iron by the smelters, or Strabo may have assimilated a technical word strange to him to a familiar one: on p. 77 Forbes himself mentions a 'tribe of iron-smiths . . . who call copper "red-iron", brass "yellow-iron"'. The present writer prefers to agree with Leaf and with Oliver Davies that metallic zinc was known in antiquity.

Chapter x on Copper is rightly the longest in the book, though confined, as already mentioned, to the Near East. Even so, the bibliography runs to 128 entries; and a vast amount of useful information has been worked into the text. Although accounts of the working of copper in Britain are excluded, recent papers by Smythe, Whittick and other English authors deserve mention for the light thrown on ancient copper alloys.

The chapter on Iron is incondite and difficult to follow. The suggestion that the earliest iron of the Near East, sometimes referred to as 'sporadic iron' was 'a by-product of the refining of gold' (p. 403) is interesting and will be new to most readers. On the vexed question of Roman cast-iron it is impossible to acquit Forbes of carelessness involving injustice to English workers. He has apparently not seen T. May's important work on *Warrington's Roman Remains* (1904), but mentions only a reference to it in Wyndham Hulme's review of Rickard's *Man and Metals* in *ANTIQUITY*, vol. VII. The Roman cast-iron found at Tiddington near Stratford-on-Avon, published, with convincing micro-photographs, by Fieldhouse, May, and Wellsford in 1931 is ignored, though the same review in *ANTIQUITY* mentions it, and concludes that 'it is clear that the Roman metallurgists could produce cast-iron and were acquainted with its properties', a verdict with which I wholly agree. Despite this blemish there is a vast amount of accurate information scattered through the chapter: a summary on p. 414 (the chapter ends on p. 467) should be digested by all archaeologists:—'Technically speaking we must, therefore distinguish the following phases of iron industry. First a period of the use of meteoric iron, then iron produced as a by-product of gold-refining, followed quickly by the evolution of the reduction of iron ores in bloomery and shaft-furnaces, which rose to prominence when methods of cementation allowed the manufacture of steel. For only steel is definitely better than bronze for tools and weapons and only the invention of steel could herald the Iron Age'.

It is because *Metallurgy in Antiquity* is a unique and indispensable work of reference that we have thought it worth while to criticise so many minor points. Is it too much to hope that Forbes himself will be able to revise it?

C. N. BROMEHEAD.

ARREST AND MOVEMENT. By H. A. GROENEWEGEN-FRANKFORT. *Messrs Faber and Faber Ltd., Russell Square, London, W.C. 1, 1951. pp. 222, pls. 96, and many line drawings. Price 50s.*

Mrs Frankfort has written an interesting and challenging book which bears evidence of much careful research and intensive thought. Her training in philosophy leads her to view her subject from an unusual angle and, if the phraseology she employs makes it somewhat difficult for the ordinary reader to comprehend her meaning at times, this should not deter him from appreciating her enlightening interpretation of many aspects of art in the Near East.

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We feel in studying the artistic products of these peoples, so remote from our own world, that we are in danger of attributing a modern viewpoint in appraising their merits or demerits. Mrs Frankfort is aware of this when she says (p. 11) 'the interpreter is in constant danger of reading into a scene what he wants to deduce from it and of explaining the absence or presence of features which do not tally with his interpretation as artistic incompetence'. That she does not always avoid this danger, which of course is very difficult for a modern critic to do, is not surprising.

We must not forget that what we term 'artistic' or 'beautiful' had very little meaning for the people she writes about, or for any other primitive community. Their paintings and sculptures served definite purposes and their beauty, when we feel it in particular instances, was only incidental and unconscious. Nevertheless individual artist-craftsmen show a superiority above their fellows and thus appear to be possessed of a higher sense of beauty than the rest.

The scenes at Beni Hasan are described as 'very dull' but allowance should be made for the inadequate reproductions of the Egypt Exploration Society's volumes. These are on so small a scale that most of the interesting details are lost. The starveling herdsman illustrated on pl. xxva has his counterpart in paint at Beni Hasan¹. It is true that much is crude and provincial in the paintings (which are obviously by different hands), but the artists who painted the birds in the mimosa bush² as well as many other birds and animals show a sense of form and colour which is quite extraordinary in any period.

The statement on p. 78 that Antefoker is the only remaining Middle Kingdom tomb at Thebes must be modified. The tombs of Daga³ and Djar⁴, both of that period, contain a number of extant scenes.

Her interpretations of the scenes on the walls of New Kingdom tombs sometimes invite queries. Granted that the deceased in Old Kingdom representations is shown merely 'watching' (p. 31) activities as a kind of passive ghost, this hardly seems to apply to many episodes in the New Kingdom. Shooting at game or fishing and fowling by the owner of the tomb may or may not have been definite events in time; with the Egyptian tendency to exaggeration many more animals and birds than he could have hoped to see together in his lifetime are provided, but he is evidently taking an active and joyful part in hunting them. The pastime is reminiscent of his days on earth although the magical element may not have been altogether absent⁵. There are however instances where such scenes are purely imaginary—a kind of wishful thinking. Tutankamūn certainly did not fight Nubians or Syrians nor slaughter so many lions as he is shown doing on his casket. But possibly this was flattery on the part of the artists; we are very familiar with the exaggeration of the king's prowess everywhere in the New Kingdom.

It has to be borne in mind that the tombs were begun during the life-time of the owner and he had the superintending of the pictures arranged on the walls. These scenes in which he was personally interested would naturally be depicted there and have a biographical meaning. When he died his tomb, in many instances, was left unfinished and it is interesting that the walls on which funerary scenes are generally pictured are those which are often left blank. The heirs, apparently, did not trouble to add the necessary burial rites closely connecting the deceased with the Hereafter.

¹ *M.M.A. Bull.*, Sect. II, 1931-2, Figs. 9, 10.

² *Ancient Egyptian Paintings*, pl. ix.

³ Davies, *Five Theban Tombs*, pls. XXIX-XXXVIII.

⁴ Winlock, *Excavations at Deir el Bahri*, pl. 17.

⁵ See Sir Alan Gardiner's remarks on this subject in *The Tomb of Amenemhēt*, pp. 14-20, pp. 38-9.

We must not forget how much has perished in the tombs and what is lost (guessed at by traces), often belonged to scenes of more interest than the commonplace survivals. Not only are iconoclasts responsible for this but contemporary enemies of the tomb-owners. For example no full-faced musicians have survived in the tombs owing to the prejudice of modern Egyptians against the evil eye: had the picture in the British Museum (pl. xxxiva), not been taken away from the necropolis soon after it was discovered that too would have been mutilated. The full-face was not so rarely pictured; at least one other 18th Dynasty example (in tomb 249), can be added to those on Wreszinski's list.

Scenes said to be 'unique' have a tantalizing habit of reappearing and it is perhaps rash to say that the garden in Sebekhotep (p. 90) is the only one of its kind when parts of similar designs can be traced elsewhere.

Personal and actual experiences do not seem so unusual as Mrs Frankfort suggests. The scene she cites on p. 80 where Amenemhet is fighting a hyena is surely to be attributed to him since his title (*hry*) *n pdt*, is inscribed above the beast⁶ and is found twice elsewhere in connection with him⁷; so there seems no reason to doubt he is identified with the action. The receiving of the Puntites by the owner of tomb 143⁸ and the landing of the Syrians from their ships to barter with the Egyptians in tomb 162⁹, are perhaps only a few of such occurrences in which the biographical element, called in question on pp. 79-80 is present.

On pp. 73, 84-5, some interesting comparisons are drawn between hunting scenes in Egypt and Assyria illustrated by Figs. 11, 14, and pls. LXXIXa, LXXIXb. The former are composed with the help of ground-lines and the latter without them.

Perhaps the reason why battle scenes are not found in private tombs of the New Kingdom (as noted on p. 88), is that, since the king is always the dominating figure in them, it would be usurping the glory of the Pharaoh vanquishing his foes to record them elsewhere than on temple walls (which were his prerogative), or on his personal objects. The private person had a very subordinate part in his pictured victories.

In the appraisal of Egyptian art of this period the freely-rendered and unconventional ink drawings on ostraca and on walls should not be ignored. Madame Vandier-d'Abbadie has excellently reproduced many of these¹⁰. Others, such as those found by Mr Winlock¹¹, are illuminating as to what the Egyptians could do when untrammelled by convention.

Turning to the chapters on Mesopotamia, one feels the early sculptures, especially those on pls. LVI, LVIII and LIX, are almost repellent in their ugliness. They unfortunately remind one of the unhappy products of some artists of our own time!

It is very different when we come to Assyrian art from about 800 B.C., when Egypt was already decadent. There low-relief sculpture attained a level of superb excellence and skill; characterization of form and movement of wild animals could scarcely be better done. Mrs Frankfort knows so much about Mesopotamian culture that her comparisons with Egyptian art are of great interest. It might have been stressed that the introduction of the horse and chariot in the time of the Hyksos was responsible for the appearance of fresh elements in Egyptian compositions from the reign of Tuthmosis III onwards.

⁶ *J.E.A.*, vol. xxvi, 1940. ⁷ Sethe, *Urk*, 906aa, 900i.

⁸ *M.M.A. Bull.*, Sect. II, 1934-5, pp. 46-48.

⁹ *J.E.A.*, vol. xxxiii, 1947, pl. viii.

¹⁰ J. Vandier d'Abbadie, *Catalogue des Ostraca figurés de Deir el Medineh*.

¹¹ H. E. Winlock, *Excavations at Deir el Bahri*, pl. 92.

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The section on Cretan art which ends the book, is perhaps the most suggestive and illuminating. It is headed the 'Cretan Enigma' and, in truth, can be termed nothing else. Its origin and its end are both obscure and in its manifestations we are aware of an inherent decadence which is baffling. Its isolation from Egyptian and Near Eastern cultures is well illustrated in those pages. On p. 186 it is justly noted that in contrast with Egyptian and Mesopotamian art, Cretan artists 'avoid depicting human achievement whether in the modest sense of manual work' or 'in the challenging one of battles won'. It might be added that the absence of any representations of ships is strange in an island community which had relations with the mainland. It is emphatically true that the feeling for beauty of form and design and the translating of material objects into lovely patterns has seldom been so satisfactorily attained. On p. 189 'ingenious reconstructions which are sometimes unsound' are referred to in connection with the paintings: Too much imaginative restoration is very apt to lead astray.

In conclusion the author must be congratulated on the fine photographic plates. Some of the line drawings in the text unavoidably suffer from the greater reduction and printing on rough paper. Only in one instance is there a mis-print in their numbering; Fig. 44 on p. 212 should read Fig. 45. A.M.D.

A PICTURE-BOOK OF ANCIENT BRITISH ART. By STUART PIGGOTT and GLYN E. DANIEL. *Cambridge*, 1951. 12s 6d.

These seventy-three photographic reproductions are good value for the price asked, and it was a good idea to supplement the moderately priced (and excellent) 'introductory books on the prehistory of the British Isles' by one containing pictures only. So far as art is concerned this book achieves its purpose; the introductory essay of eleven pages gives just so much background information as the intelligent and not too ignorant reader needs, including a short summary table of absolute chronology. The explanatory list of photographs provides all the essential known facts, and, greatly daring, a date in years for each object.

A few minor observations may be recorded. The references to published accounts of the objects would have been more convenient if combined with the plate-descriptions and not printed as a separate list. As it is, one has to turn to two separate places for each illustration. Of course the proper place for both is opposite the plate itself, but one knows that that is impossible nowadays. Speaking from memory we believe the Rillaton gold cup was found in a burial-chamber, not a cist; the barrow is normal and still existed a few years ago when we visited it, and probably exists still. (It would be worth while clearing* and planning it).

Most of the earlier objects illustrated please chiefly by reason of their good craftsmanship, but from No. 37 onwards, we are well within the realm of art, and things like the Battersea shield and Birdlip mirror are really beautiful. All the latter exemplify the saying that 'genuine art is always produced for current use'. For those who are troubled by doubts about modern art we would suggest another picture-book devoted to mugs, tankards, coins, helmets and personal ornaments, carried rapidly through the Christian era and culminating in those produced for current use. On this basis the Rillaton cup, the Trawsvynydd tankard, the Ardagh chalice and a modern water-jug of moulded glass (borrowed from any restaurant) are all strictly comparable. But a similar series of coins—British, Saxon, medieval and modern Irish—would set the balance straight.

O.G.S.C.

* But *not* by the Ministry of Works—for then no account would be published.

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THE CALENDARS OF ANCIENT EGYPT. By RICHARD A. PARKER. (Studies in Ancient Oriental Civilization, no. 26). *University of Chicago Press*, 1950, 83 pp., vi pls. \$6.00.

There have been many studies dealing with the Egyptian evidence for ancient chronology; in none of them have the mysteries surrounding some known cases of a double dating, one by the civil year, and one by lunar months, been clarified. Professor Parker in this study proves that the lunar month began always with the first day on which the old crescent was no longer seen. This demonstration fortunately removes the complicated set of hypotheses resulting from calculations which used sometimes the first day of invisibility, sometimes the day of the new crescent, and confirms the coherence of the Egyptian way of reckoning time. The day started with dawn, and the observation governing the month must have been made at dawn. In proving this point, a neat and convincing explanation of the real significance of the demotic papyrus Carlsberg no. 9, published by O. Neugebauer and A. Volten, which gives a 25-year cycle of civil years in which the lunar years contain 9 intercalations, brings evidence of great importance for serious students of chronology.

The luni-stellar year originally used in Egypt can be identified with the lunar calendar of the Ebers Papyrus, and Parker ingeniously uses the Ramesseum and Senmut ceilings to develop his view that the intercalary month was named after Thoth. In an important excursus he deals with the chronology of the 12th Dynasty. Though the dates arrived at by Lynn Wood were correct, they are only so because the two errors involved, reckoning by the new crescent and attribution of certain lunar dates to the wrong reign, cancel one another.

This brochure is a masterly piece of clear exposition. Though it is hardly to be hoped that it will stop some of the flood of ill-informed writing on this subject, Parker's work leaves no excuse for some of the ill-digested schemes of chronology recently published.



ROMAN FORT, BECKFOOT, CUMBERLAND

Journal of Roman Studies, 1951, plate iv, 2

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ROMAN FORT, GLENLOCH, KIRKCUDBRIGHTSHIRE

Journal of Roman Studies, 1951, plate vii, 1

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